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ARTICLES

CHOOSING PREFERENCES BY CONSTRUCTING INSTITUTIONS: A CULTURAL THEORY OF PREFERENCE FORMATION

AARON WILDAVSKY
University of California
Berkeley

Preferences come from the most ubiquitous human activity: living with other people. Support for and opposition to different ways of life, the shared values legitimating social relations (here called cultures) are the generators of diverse preferences. After discussing why it is not helpful to conceive of interests as preferences or to dismiss preference formation as external to organized social life, I explain how people are able to develop many preferences from few clues by using their social relations to interrogate their environment. The social filter is the source of preferences. I then argue that culture is a more powerful construct than conceptual rivals: heuristics, schemas, ideologies. Two initial applications—to the ideology of the left-right distinctions and to perceptions of danger—test the claim that this theory of how individuals use political cultures to develop their preferences outperforms the alternatives.

> The question of where political ideas come from is not only highly deserving of study, but also within the competence of our contemporary research techniques. I join Bill Riker in commending it to you as one of the truly exciting and significant areas of investigation in our field.
>
> —Herbert Simon

> Agreement on political fundamentals cries for an explanation. Why, how, through which mechanisms do people come to think alike about political fundamentals?
>
> —Charles E. Lindblom

The formation of political preferences ought to be one of the major subjects of political science. Although it is eminently reasonable to study—as most of us, including myself, have throughout our professional lifetimes—how people try to get what they want through political activity, it is also *unreasonable* to neglect the study of why people want what they want. To omit or slight the most important reason all of us have for studying politics, namely, educating our preferences, is a particularly unfortunate lapse for scholars.

I am making a double argument: first, on behalf of the usefulness of a cultural approach in general (rooting explanation in social life) and, second, on behalf of a particular cultural theory (cultures characterized by boundedness and prescription). Readers might find the first more persuasive than the second. There may be

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better formulations. My brief for the cultural theory that follows is based upon the usual criteria of parsimony and power, that is, getting the most explanatory and predictive capacity from the fewest variables. Challenges and improvements are welcome.

Interests As Preferences

Ask political scientists where preferences come from and if they don't just stop the conversation with "haven't a clue" or refer disparagingly to the muddle over ideology, you are likely to hear that ubiquitous catch-all term "interests." Preferences presumably come from the interests people have. Indeed, a sweeping review of the literature done by Michael Thompson and Michiel Schwarz (1985) tells us what we already suspect: politics of interests is the mainstay of political science.[1] Yet, if preferences come from interests, how do people figure out what their interests are (presumably, these do not come with a birth certificate or social security card) so they will know what they prefer? For if interests and preferences are synonymous, we still are no wiser about how people come to have them.

In the beginning, apparently, there were interests—lumpy, fully formed psychological facts, self-evident and self-explanatory. How any one of us would come to know what our interests are need not matter if they are derived from immediate sense perception. Individuals, presumably, size up the situation, distinguish opposing interests, separate the interests of others from self-interest, and choose (or choose not to choose) the self. Instead of this phenomenological understanding—interests are self-evident, chiseled in stone on objects that force themselves as they are upon human perception —I would rely on the convergence of certain strands of work in social science, according to which *meanings are shared;* they are conferred on objects or events through social interaction.

If the interests that we consider ours are indeed the products of social relations, then the origins of our preferences may be found in the deepest desires of all: how we wish to live with other people and how we wish others to live with us. "The real moment of choosing," as Mary Douglas (1983) maintains, "is . . . choice of comrades and their way of life" (p. 45). But that fateful choice, while broad, is not unlimited.

The first choice—the available combinations of values and practices—is made *for* us. Human beings do not choose what they want, like ordering a la carte, any more than they (so far) select their body parts in any size or shape they want, regardless of the configuration into which these have to fit. Preference formation is much more like ordering prix fixe from a number of set dinners or voting a party ticket. Only those combinations that are socially viable, that can cohere because people are able to give them their allegiance, to share their meanings, may be lived. Some things—accepting authority while rejecting it—just can't be done. Only second-level choices (which of the available ways of life do I prefer?) and third-level choices (which policies do I believe are efficacious in supporting my preferred way?) are potentially available to choice. If preferences are formed through the organization of social relations, however, these preferences must come from inside, not from outside, our ways of life—from institutional arrangements.

Preferences Are Endogenous, Not Exogenous

Ask an economist where preferences come from and you will be told that they are exogenous, external to the system being considered.[2] The motive force for participation in markets, the desire to do better through living a life of bidding and bargaining by competing for resources, is ruled out as a noneconomic question! Worse still, preferences are referred to as "tastes," for which, as the saying goes, there is no accounting, thus rendering them not merely noneconomic but nonanalyzable.

The difficulty for economics conceived as rational choice is stated cogently by R. T. Michael and G. S. Becker (1976):

> For economists to rest a large part of their theory of choice on differences in tastes is disturbing since they admittedly have no useful theory of the formation of tastes, nor can they rely on a well-developed theory of tastes from any other discipline in the social sciences, since none exists. . . . The weakness in the received theory of choice, then, is the extent to which it relies on differences in tastes to "explain" behavior when it can neither explain how tastes are formed nor predict their effects. (in Burt 1982, 347–48)

Nevertheless, Becker goes on to state that "all human behavior can be viewed as involving participants who maximize their utility from a stable set of preferences" (p. 348). If preferences are fixed and outside the process of choice, then we cannot inquire into how preferences are formed. The least interesting behavior, instrumental actions, may be explained by preferences; but about the most interesting, preferences themselves, nothing at all can be said. Lindblom is right: "We have impoverished our thought by imprisoning it in an unsatisfactory model of preferences taken as given" (1982, 335).

Cultural theory, by contrast, is based on the premise that preferences are endogenous—internal to organizations—so that they emerge from social interaction in defending or opposing different ways of life. When individuals make important decisions, these choices are simultaneously choices of culture—shared values legitimating different patterns of social practices. Always, in cultural theory, shared values and social relations go together: there are no disembodied values apart from the social relations they rationalize, and there are no social relations in which people do not give reasons for or otherwise attempt to justify their behavior. When choices are not completely controlled by conditions (cultural theory holds), people discover their preferences by evaluating how their past choices have strengthened or weakened (and their future choices might strengthen or weaken) their way of life. Put plainly, people decide for or against existing authority. They construct their culture in the process of decision making. Their continuing reinforcement, modification, and rejection of existing power relationships teaches them what to prefer.

"If political preferences are molded through political experiences, or by political institutions," James March and Johan Olsen (1984) state, "it is awkward to have a theory that presumes preferences are exogenous to the political process" (p. 739). Cultural theory, by contrast, gives preferences an endogenous political explanation: preferences are formed through opposing and supporting institutions.

Rejecting a social science that begins at the end by assuming interests, I wish to make *what people want*—their desires, preferences, values, ideals—into the central subject of our inquiry. By classifying people, their strategies, and their social contexts into the cultural biases that form their preferences, cultural theory attempts to explain and predict recurrent regularities and transitions in their behavior. Preferences in regard to political objects are not external to political life; on the contrary, they constitute the very internal essence, the quintessence of politics: the construction and reconstruction of our lives together.

Deriving Preferences from Cultures: Four Ways of Life

Cultural theory is based on the axiom

Figure 1. Models of Four Cultures

Number and Variety of Prescriptions	Strength of Group Boundaries	
	Weak	**Strong**
Numerous and varied	Apathy (Fatalism)	Hierarchy (Collectivism)
Few and similar	Competition (Individualism)	Equality (Egalitarianism)

Note: Adapted from Douglas 1970; 1982.

that what matters most to people is their relationships with other people and other people's relationships with them. It follows that the major choice made by people (or, if they are subject to coercion, made for them) is the form of culture—shared values legitimating social practices—they adopt. An act is culturally rational, therefore, if it supports one's way of life.

A basic proposition of this cultural theory (which cannot be demonstrated here) is an impossibility theorem: there are only a limited number of cultures that between them categorize most human relations.[3] Though we can imagine an infinite number of potential cultures, only a relatively small number (here I shall work with four) are filled with human activity; the rest are deserted. What makes order possible is that only a few conjunctions of shared values and their corresponding social relations are viable in that they are socially livable.

The dimensions of cultural theory are based on answers to two questions: Who am I? and What shall I do? The question of identity may be answered by saying that individuals belong to a strong group, a collective, that makes decisions binding on all members or that their ties to others are weak in that their choices bind only themselves. The question of action is answered by responding that the individual is subject to many or few prescriptions, a free spirit or a spirit tightly constrained. The strength or weakness of group boundaries and the numerous or few, varied or similar, prescriptions binding or freeing individuals are the components of their culture.

Strong groups with numerous prescriptions that vary with social roles combine to form hierarchical collectivism. Strong groups whose members follow few prescriptions form an egalitarian culture, a shared life of voluntary consent without coercion or inequality. Competitive individualism joins few prescriptions with weak group boundaries, thereby encouraging ever new combinations. When groups are weak and prescriptions strong—so that decisions are made for them by people on the outside—the controlled culture is fatalistic (See Figure 1).

The social ideal of individualistic cultures is self-regulation. They favor bidding and bargaining in order to reduce the need for authority. They support equal opportunity to compete in order to facilitate arrangements between consenting adults with a minimum of external interference. They seek opportunity to be different, not the chance to be the same, for diminishing social differences would require a central, redistributive authority.

Hierarchy is institutionalized authority. It justifies inequality on grounds that specialization and division of labor enable people to live together with greater harmony and effectiveness than do alternative arrangements. Hence, hierarchies are rationalized by a sacrificial ethic: the parts are supposed to sacrifice for the whole.

Committed to a life of purely voluntary association, those from egalitarian cultures reject authority. They can live a life without coercion or authority only by greater equality of condition. Thus egalitarians may be expected to prefer reduction of differences—between races, or income levels, or men and women, parents and children, teachers and students, authorities and citizens.

An apathetic culture arises when people cannot control what happens to them. Because their boundaries are porous but the prescriptions imposed on them are severe, they develop fatalistic feelings: what will be, will be.[4] There is no point in their having preferences on public policy because what they prefer would not, in any event, matter.

But none of these modes of organizing social life is viable on its own. A competitive culture needs something—the laws of contract—to be above negotiating; hierarchies need something—anarchic individualists, authority-less egalitarians, apathetic fatalists—to sit on top of; egalitarians need something—unfair competition, inequitable hierarchy, nonparticipant fatalists—to criticize; fatalists require an external source of control to tell them what to do. "What a wonderful place the world would be," say the adherents of each culture, "if only everyone were like us," conveniently ignoring that it is only the presence in the world of people who are not like them that enables them to be the way they are. Hence, cultural theory may be distinguished by a necessity theorem: conflict among cultures is a precondition of cultural identity. It is the differences and distances from others that define one's own cultural identity.

Alone, no one has power over anyone. Power is a social phenomenon; power, therefore, is constituted by culture. But the form and extent of manipulation vary. Apathetic cultures are manipulated; fatalists live by rules others make and impose upon them. Manipulation is built into hierarchies; orders come down and obedience presumably flows up. The evocative language of New Guinea anthropology (the "big men" versus the "rubbish men") expresses the growth of manipulation in market cultures as some people cease to possess the resources to regulate their own lives. Egalitarians try to manipulate the other cultures by incessant criticism; they coerce one another by attributing inequalities to corruption and duplicity.[5]

To identify with, to become part of a culture, signifies exactly that: the unviable void of formlessness—where everything and therefore nothing is posible—is replaced by social constraint. Even so, individuals keep testing the constraints, reinforcing them if they prove satisfactory in practice, modifying or rejecting them, when possible, if unsatisfactory. It is individuals as social creatures, not only being molded by but actively molding their social context—shaping the maze as well as running it—that are the focus of cultural theory.

Suppose a new development occurs. Without knowing much about it, those who identify with each particular way of life can guess whether its effect is to increase or decrease social distinctions, impose, avoid, or reject authority—guesses made more definitive by observing what like-minded individuals do.[6] Of course, people may be, and often are, mistaken. To seek is not necessarily to find a culturally rational course of action. Gramsci's would-be capitalists may try to establish hegemony over others, but they are often mistaken about which ideas and actions will in fact support their way of life. They may, for instance, use governmental regulation to institute a pattern of cumulative inequalities that convert market arrangements into state capitalism, leading to their ultimate subordination. To be culturally rational by bolstering one's way of life is the intention, not necessarily the accomplishment.

If social life is the midwife of political preferences, how do people get from culture to preferences? Perhaps politics is too complicated to allow many people to figure out what they prefer.

"Preferences Need No Inferences"

An obstacle to the development of a theory of political preference formation is the view, dominant in psychology until recently, that cognition must precede affect. For if "preferences are formed and expressed only after and only as a result of considerable cognitive activity" (Zajonc 1980, 154), then it would indeed be difficult to explain how most people, including many who engage only in minimal cognitive activity, at least in regard to politics, come to have so many preferences. If, however, one goes along with Zajonc and the considerable literature he cites "that to arouse affect, objects need to be cognized very little—in fact minimally" (p. 154), more promising theoretical avenues open up. Preferences, Zajonc continues, "must be constituted of interactions between some gross object features and internal states of the individual" (p. 159). But how, we may ask, do preferences get from object features to internal states?

The cultural hypothesis is that individuals exert control over each other by institutionalizing the moral judgments justifying their interpersonal relationships so they can be acted upon and accounted for. The prevailing view is that the interrelatedness among attitudes in the mass public is low, that is, people are inconsistent. Now, criteria of consistency expressing what ought to be related to what are not found in nature but, like the categories of culture I am expounding, are imposed in an effort to make sense out of people's political behavior. If these criteria are incorrectly or insufficiently specified, they will make people's opinions unrelated where another set of criteria would make them more consistent. When there is a question as to whether it is the people who do not understand what they are doing or we social scientists who do not understand the people, I am inclined to think that *we* have fallen down. All of us in social science are looking for bedrock, for the most basic value and factual premises that we can hypothesize as lying behind specific political and policy preferences. My claim is that this foundation lies in social relationships, roughly as categorized by political cultures.

foundation lies in social relationships, roughly as categorized by political cultures.

How does the social filter enable people who possess only inches of facts to generate miles of preferences? What is it about cultures that makes them the kind of theories that ordinary folk can use to figure out their preferences? The ability of people to know what they prefer without knowing much else lies at the crux of understanding preference formation. Culture codes can be unlocked, I maintain, because its keys are social. By figuring out their master preferences, as it were—who they are and are not, to what groups they do and do not belong—they can readily figure out the rest. A basic reason people are able to develop so many preferences is that they actually do not have to work all that hard. A few positive and negative associations go a long way.

It is no more necessary for a person to verbalize about culture than it is necessary to know the rules of grammar in order to speak. The stock phrases "one of us" versus "one of them" goes a long way. Preferences might come from insight into general principles, but, because meanings have to be shared, ideologues and theorists often discover that their views are rejected or modified by others. Preferences can and do come sideways, from identifications, experiences, and conversations. What matters is not how preferences are first proposed (many are called but few are chosen) but how they are ultimately disposed through the presence or absence of social validation. It is not the lone individual, after all, who creates what is called *ideological constraint* ("one thing entailing another") among preferences but social interaction among adherents of a particular culture in contrast to other cultures whose identifiers have different preferences.

Heuristics

Brady and Sniderman, in pursuing a closely related question, "How . . . can citizens make sense of groups—that is, know which is relevant to which issue and which stands for what—without having to know a great deal about them?" (1985, 1073),

> focus on the operation of an affective calculus, or, as we call it, a *likability heuristic*. This calculus is organized around people's feelings toward groups such as liberals and conservatives. Clearly, many in the mass public lack a firm understanding of political abstractions. All the same, many know whom they like, and, equally important, they also know whom they dislike. If coherent, these likes and dislikes can supply people with an affective calculus to figure out the issue positions of strategic groups. We suggest that in this way many in the mass public can figure out who wants what politically without necessarily knowing a lot about politics. (pp. 1061–62)

The more people are able to choose sides —ours versus theirs—"the more they appreciate the differences between the issue positions of the two sides. What counts, then, is not how people feel toward groups, one by one; rather it is how they feel toward *pairs* of opposing groups" (p. 1075). It is precisely this pairing or, more accurately, this triangulation of rival cultures, I believe, that enables people to position themselves in political life.

Preferences may be rationalized from the top down, specific applications being deduced from general principles. But complexity of the causal chains invoked leaves people who lack a capacity for abstract thought unable to form preferences. Reasoning in steps is also slow. Without social validation at each step, moreover—which is difficult to achieve—the chain of reasoning may snap. Fortunately, faster methods are available. People can know what they believe or whom they trust without knowing how the belief is derived. Sniderman, Hagen, Tetlock, and Brady (1986) agree that such bottom-up processes operate on white attitudes toward blacks. In their view

> It may be nearer the mark to say that citizens, so far as their reasoning about policy is affect-driven, start at the beginning of the chain, taking account of their feelings toward blacks. Then, rather than working their way along the chain hierarchically, from general to specific, they skip over the intermediate links of the chain and go straight to its end. Having reached the end of the chain, they work their way backwards and fill in the missing links. That is to say, not only do they reason forwards, from general to specific; they also reason backwards, from specific to general. And, because they can reason both forwards and backwards, with affect guiding them, they can indeed figure out what they think about questions, such as the reasons for racial inequality, they may not ordinarily think about. (p. 33)

Mediating their perceptions through their cultures, people can grab on to any social handle to choose their preferences. All they need are aids to calculation.

"How," Paul Sniderman and his colleagues ask, "do people figure out what they think about political issues, given how little they commonly know about them?" (Sniderman et al. 1986, 2). They state that "three heuristics are of particular importance: affect (likes and dislikes); ideology (liberalism/conservatism); and attributions of responsibility (the so-called desert heuristic)" (p. 2). The desert heuristic is a version of system versus individual blame through which adherents of political cultures seek to hold others accountable for their behaviors. "Liberal" versus "conservative" stands as a surrogate for equality of condition versus equality of opportunity, that is, for the rivalry of egalitarian and market cultures. (When "liberal" meant "laissez-faire," its cultural associations were different.) The two heuristics—desert and ideology—are related: market forces blame individuals (they are undeserving); egalitarians blame the system (it is oppressive). Liberals dislike conservatives because they "blame the victims," while conservatives dislike liberals because they encourage irresponsible behavior. All these aids to calculation are ideological (or, to use the anthropological term, cosmological) in the sense of *rationalizations* for preferred social relationships.

I agree entirely that

> it would be . . . a mistake merely to enumerate various heuristics; a mistake partly because they are likely to proliferate endlessly; a mistake more fundamentally because it is necessary to understand how these aids to judgment are themselves interrelated. It is, that is to say, necessary, to understand how people work their way, step by step, through a chain of reasoning. And to understand how they manage this, one must establish what they do first, then second, then third. (Sniderman et al. 1986, 47)

Cultural theory attempts to unify heuristics by suggesting that these chains have but one link: the internalization of external social relations.

Schemas

Another entry for understanding the formation of political preferences has now appeared—schema theory. According to Pamela Conover and Stanley Feldman (1984), this theory views "people as 'cognitive misers' who have a limited capacity for dealing with information, and thus must use cues and previously stored knowledge to reach judgments and decisions as accurately and efficiently as possible" (p. 96). Political cognition is about how different schemas, such as party identification and economic class, organize perception. How are the schemas that form our preferences formed?

Although the logic of schemas may appear similar to that of cultural theory—a small number of premises generating a large number of premises—this appearance is misleading. Cultures are not disembodied ideas; they are not merely cognitive.[7] The mental activity has a purpose: the justification of desired social practices. It is both together, shared values indissolubly connected to social practices, that make up cultural theory. Comparing cultures means just that—comparing cultures as totalities with values and practices joined, not isolated.

The concept of schemas, essentially a reinvention of our old friend "attitudes" under a new name (like "political behavior" for "political science"), falls prey to the same disability—the endless proliferation of explanatory constructs until there is an attitude or a schema for every act. I think that the notion of schemas lacks a crucial element that cultural theory offers: a systematic context from which preferences can flow. Let us try a couple of quick tests.

Two Tests of Cultural Theory: Ideology and Risk

Cultural theory is open to tests normally applied in social science: *retrodiction* (Can it explain historical puzzles? [Ellis and Wildavsky 1986]) and *prediction* (Does it account for future events better than do other theories?). The degree of incorporation into group life and the degree of prescription can be measured so as to arrive at (forgive the cumbrous expression) intersubjective coder reliability. Jonathan Gross and Steve Rayner's book, *Measuring Culture,* does just that.

One test of cultural theory is conceptual-historical; I contend that the cultural categories described here fit far better in accounting for political preferences than the usual left-right, liberal-conservative dimensions. A second test is both contemporary and future-oriented; I claim that perception of danger and disposition toward risk—from technology and from acquired immune deficiency syndrome—are better explained and predicted by cultural theory than by competing theories.

A Confusion of Cultures: Competitive Individualism versus Egalitarian Collectivism

The single worst misunderstanding about U.S. politics, in my opinion, is the joining together as a single entity, called "individualism," two separate and distinct political cultures with opposing preferences for policies and institutions—competitive individualism and egalitarian collectivism. Between equality of opportunity (enabling individuals to accentuate their differences) and equality of results (enabling them to diminish their differences), there is a vast gulf.[8] To say that equal opportunity is empty without more equal results is to say that the latter is more important than the former.

Individualistic cultures prefer minimum authority, just enough to maintain rules for transactions, but they do not reject all authority; if it leaves them alone, they will leave it alone. While egalitarians also like to live a life of minimal prescription, they are part and parcel of collectives in which, so long as they remain members, individuals are bound by group decisions. This critical distinction in group-boundedness, the freedom to transact for yourself with any consenting adult vis-à-vis the requirement of agreement with group decisions, makes for a radical difference in the formation of political preferences.

The confusion to which I am objecting manifests itself more generally in the use of dichotomous instead of triangular designations of political cultures. The most infamous of these is left versus right. Left, or liberal, presumably designates a tendency toward greater use of central government for policy purposes, including an inclination to welfare state measures designed to be at least somewhat redistributive. Presumably, right, or conservative, signifies a disposition against central governmental intervention in the economy but of greater respect for collective authority. As political shorthand, these terms have their uses. But for purposes of political analysis, they obfuscate more than they clarify. The preference for greater use of government may stem from a hierarchical culture in which the individual is subordinated to the group. Yet the very same preference for central governmental action may be rooted in a desire to reduce all social distinction, including those on which hierarchies are based. Hierarchies and egalitarian collectives may, in certain historical contexts, ally themselves in favor of redistributive measures, yet they may also, at the same time, be bitter opponents in regard to respect for authority. For equalization of statuses would destroy hierarchy. It is not easy, as the Catholic Church is learning, to say that all forms of inequality are bad but that popes and bishops are good (Wildavsky 1985a).

The left-right distinction is beset with contradictions. Hierarchical cultures favor social conservatism, giving government the right to intervene in matters of personal morality. Thus egalitarians may support intervention in the economy to reduce economic differences but not intervention in social life to maintain inequality. Libertarians, who are competitive individualists, oppose both social and economic intervention.

A division of the world into left and right that is equally inapplicable to the past and to the present deserves to be discarded. Efforts to read back the left-right distinction into U.S. history, for instance, succeed only in making a hash of it. In the early days of the republic, egalitarians pursued their objectives through severe restrictions on central government because they then regarded the center as monarchical, that is, hierarchical. Nowadays, after decades of dispute and struggle, they regard the federal government as a potential source for increasing equality. Their egalitarian objectives remain constant, but their beliefs about what will be efficacious instruments of policy vary according to the conditions of the times (cf. Banning 1978).

Without knowledge of the historical context, and therefore, without being privy to the internal discussions through which shared meanings are worked out, it is impossible to explain why a given culture prefers certain institutional arrangements and instruments of policy at one time and different ones on other occasions. How, nowadays, make sense of the Republican alliance of economic free markets and social conservatism or the Democratic combination of statism with distrust of authority? Is it the "left" that supports the authority of central government and the "right" that opposes it, or is it the "right" that respects authority and the "left" that denigrates it?

The division of the political universe into liberals and conservatives, when based on innate tendencies toward change, is bound to be misleading because historical context alters whatever the various political cultures wish to preserve. Given the current extent to which most proposals for government action involve redistribution of income or regulation of business, it is not surprising that people who are opposed to these policies have learned to dislike change. So, when asked, they reply that most change is for the worse. People who prefer these programs respond that they like change. Were the tables turned, so that most legislation was in favor of maintaining social and economic differences, say anti-abortion and anti-inheritance taxes, contemporary liberals would learn that most change is bad and their conservative opponents that change is by and large good.

In a rich analysis of differences and similarities among left- and right-wing activists, McClosky and Chong (1985) conclude that "thus, paradoxically, despite its patriotic fervour, spokesmen of the radical right are profoundly antagonistic to the status quo" (pp. 346–47). It is paradoxical if conservatism is identified with resistance to change but not if desire for change depends on perceived distance from desired behavior. Those who look at life from the conservative perspective "continually lash out against what they consider to be the government's conciliatory stance towards Communism, its support for welfare programmes, (which, in their view, rewards laziness and lack of initiative), its encouragement of moral depravity (sexual license, tolerance of abortion, homosexuality, etc.), and its lenient treatment of criminals" (pp. 346–47). If readers believed that, they might also want big changes. What kind of changes we want depends not nearly so much on our predispositions toward change per se, as if the destination did not matter, but on the gap between desired and actual power relationships.[9]

The further the distance between the real and the ideal, the greater the desire for rapid and radical change. If this proposition is correct, it should follow that "left" or "progressive" forces, when they consider existing power relationships more desirable than proposals for change, should cling to the status quo with as much passion as any reactionary who prefers the last century to the present. Wandering in the void between the Articles of Confederation (interpreted as minimal central authority) and the Constitutional Convention (which, by comparison, elevated central power), the antifederalists preferred the past to the future. Worrying about the return of monarchy or, just as bad, monarchical principles, the individualist Jacksonians (who believed that equality of opportunity, rigorously enforced, would lead to relative equality of condition), fought a rearguard action against commercial capitalism.[10] Similarly, the Federalist party (a hierarchy coalescing with market forces to form an establishment) fought to achieve and maintain the relative centralization of the Constitution—a radical change from the immediate past.

An advantage of cultural theory is that it handles both economic and social issues without strain. Conover and Feldman (1981) wrote that

> traditionally, it was assumed that the meaning of ideological labels and self-identifications could be easily summarized in terms of a single dimension: the liberal/conservative continuum. In recent years, however, this viewpoint has undergone some modification. The decade of the 1970s ushered in a variety of "social" issues—abortion, marijuana use, the Equal Rights Amendment—which did not fit easily into the traditional liberal/conservative spectrum. Because of this, many researchers now posit that the meaning of ideological labels and self-identification must be interpreted within the context of two liberal/conservative dimensions: one economic and one social. (p. 168)

Using cultural concepts, however, makes such ad hoc category massage unnecessary. Individualists, being nonprescriptive and anticollectivist, prefer minimal economic and social regulation. Egalitarians, combining nonprescription with collective decision, prefer strong economic but weak social regulation. And adherents of hierarchy, joining hard group boundaries to heavy prescription, desire strong social and economic regulation. Presumably, students of cultural theory would not be surprised at a U.S. president who (combining market individualism with social hierarchy, like his party) urges compulsory urine tests to detect drug users.

Culture and Risk

Comparing perceptions of danger is especially useful as a test of cultural theory. The subject abounds with anomalies; it is fiercely contested; rival theories are already in place; and, best of all, readers can check out the performance of cultural theory vis-à-vis its competitors by reading their daily newspapers.

"Ideology," Samuel Barnes (1966) reminds us, "is one of the most frequently cited and inadequately understood subjects of empirical inquiry" (p. 513).[11] In an effort to improve the situation, a number of anthropologists (Claude Levi-Strauss, Clifford Geertz, David Schreider, Ward Goodenough) have brought up the concept of cultures as "ideational codes" (Elkins and Simeon 1979). For Geertz, "culture is best seen not as complexes of concrete behavior patterns—customs, usages, traditions, habit clusters . . . but as a set of control mechanisms—plans, recipes, rules, instructions (what computer engineers call 'programs')—for the governing of behavior" (Elkins and Simeon, 1979, 129). In order to give greater precision to this research program, Elkins and Simeon list a number of questions (regarding people's assumptions about causality, human nature, the "orderliness of the universe") that would get at who is controlling whom or what controlling which. Let us take one of their most explicit questions—"Should one try to maximize gains, or to minimize losses? In other words, what assumptions are made about the relative payoffs of optimistic or pessimistic strategies?" (p. 132)—and compare the kind of answers given by cultural theory with the kind given by other theories of risk-taking and risk-aversion.

In discussions of technological danger, one theory is that people are reacting to the actual dangers; they are risk-averse because the risks are rising. Another theory is psychological; there are risk-taking and risk-averse personalities. Still another theory concerns an intuitive sense of justice: people are willing to accept dangers that are voluntarily undertaken, but they reject risks that are imposed on them. In *Risk and Culture* (1982), Mary Douglas and I argue that perception of danger is a function of political culture, risk acceptance going along with approval of individualistic and hierarchical cultures and risk aversion with egalitarian opposition to these other cultures on the grounds they are coercive and domineering. Put briefly, we contend that the debate over risk stemming from technology is a referendum on the acceptability of U.S. institutions. The more trust in them, the more risk acceptance; the less trust, the more risk rejection.[12]

Consider, in this context of competing explanations, a variety of survey findings. The first, a survey of the feelings of a variety of elites about the safety of nuclear power plants shows, among other things, an immense gap (far greater than survey research usually produces) between nuclear energy experts (98.7%) and the military (86.0%) saying "safe" compared to relatively tiny proportions of leaders of public interest groups (6.4%), movie producers and directors (14.3%) and elite journalists (29.4%). The difference between people expected to support and to oppose authority is very great (Rothman and Lichter 1985).

A second poll compares the general public to executives of small and large corporations and environmentalists on a variety of preferences related to politics and public policy. Whereas around two-thirds of the general public and executives favor a strong defense, only a quarter of environmentalists give it a high priority. Maintaining order in the nation gets around 80% or more from everyone else but just 47% from environmentalists. On an egalitarian issue, such as having more say at work, the situation is reversed. Two-thirds of environmentalists and the general public give it a high priority but only a quarter of executives in large companies and two-fifths in small ones do the same (Bloomgarden 1983). Polarization of elites is evident.

A third study surveys business and ecology activists vis-à-vis the general public in West Germany in regard to their political positions on a left-right basis. It is obvious that ecologists and business elites are divided (twice as sharply as the general public) on ideological grounds. Similarly, in a study of voters in the United States Senate, Kalt and Zupan (1984) report, "It turns out that politicians consistently package liberalism and environmentalism together—the correlation between the LCV [League of Conservation Voters]'s and the ADA [Americans for Democratic Action]'s rating scales is 0.94." But why are the two found together?

Nuclear war may well be the greatest contemporary risk of all. Glenn Sussman (1986) has conducted a survey of U.S. and British anti-nuclear weapons activists in which he asked them to rate four goals: fighting rising prices, giving the people more say in important governmental decisions, maintaining order in the nation, and protecting freedom of speech. A priori there is no reason to believe that these activists have anything else on their minds except opposition to nuclear war. Yet approximately two-thirds valued more say in government as their first priority while maintaining order got less than 5%.[13] If one posits a cultural connection between this "anti" activism and opposition to existing authority as inegalitarian, the low ranking of "order" makes more sense. Viewing environmentalists as protestors against inegalitarian institutions (recall their concern about "endangered species" and corporations that cause cancer) helps us understand their political alliances. Because Berkeley constitutes a kind of political-medical museum for this purpose, we can observe a member of the city council, accused of

Table 1. The Double Polarization in West Germany, 1980: Left Versus Right

		"How would you describe your political attitude?"					
Respondents	(n)	Strong Left	Middle Left	Center	Middle Right	Strong Right	No Position
Ecology activists	(98)	9%	27%	29%	15%	3%	14%
General public	(1,088)	3	16	39	23	6	12
Business leaders	(130)	1	8	30	49	8	3

Source: Milbrath 1981a, 1981b; Lauber 1983.

spending too much time on foreign affairs instead of local concerns, respond, "You can't explain one without the other. If the money was not going to Central America, we would have the money to fix the sewers."[14]

Why, if we are dealing with a reasonable adaptation to emerging knowledge, do attitudes to political authority distinguish so well positions on nuclear power? Why, if there are major personality differences, do ecologists and environmentalists and businessmen divide so neatly on general ideological grounds? Why, if it is the voluntary/involuntary distinction that matters, are there such strong and similar differences on public order and defense? Rooting explanation in adherence to several different ways of life rather than the usual left-right dichotomy, I think, makes more sense out of the data.

A striking contemporary example connecting culture and risk comes from perceptions of acquired immune deficiency syndrome. The more hierarchical the group, I hypothesize, following cultural theory, the more it minimizes technological danger as the price of progress while maximizing fear of casual contact with people who have AIDS. For, in its view, when people violate divine commandments, the Lord brings plague. Conversely, egalitarians tend to grossly overestimate the dangers from technology (on grounds that the social and economic relationships they dislike are bad for your health) while minimizing the dangers from casual contact with carriers of AIDS. Gays are good in the egalitarian view because they are antiestablishment and because they reduce differences among people. Only cultural theory explains why, when we know a group's general ideology, we can tell how much danger they will impute to technology versus AIDS.

Now this conclusion, which is sure to be contested, depends on a substantial scholarly apparatus. How can laymen, that is, most of us most of the time, figure out what our preferences ought to be?

The Calculation of Preferences

How do people make so much, derive so many preferences, from so few clues? We know that most people are not interested in or knowledgeable about most issues most of the time. Consequently, the clues must be exceedingly simple. Even the highly educated and interested cannot know much about most matters of politics and policy, yet they are able to generate and express preferences when necessary. Indeed, the educated may well be getting more than their due from social scientists. Though they do know more about a few major issues than the less educated, people with high levels of formal education have many more preferences than they can know much, if anything, about. It is likely, therefore, that the highly educated have many more unfounded preferences than do those who have far fewer preferences about subject matter of which they know little.

Rational people, I have argued, support their way of life. By answering two questions, they are able to discover their cultural identity: Who am I? (free to negotiate or bound by a group?) and What should I do? (follow detailed prescriptions that vary with role or decide for myself?). Knowing who they are and are not—the cultures to which they do and do not belong—helps them to begin sorting their preferences. Cultural identity enables individuals to answer for themselves the crucial quantitative and qualitative questions about preferences: How many are they expected to have? What kind should these be? Fatalists know that they do not need to know anything (it won't matter) except what others tell them to do. They are prescribed to, not prescribing. Members of hierarchies can rule in whatever goes with their station and rule out whatever does not. By relying on others whose duty it is to take care of whatever they neglect and by positive reinforcement of this nonparticipation—it is normative not to act above (or, for that matter, below) your station—both groups come to learn how much of what kind of preferences they are expected to have and how much they can leave to the authorities. Individualists are expected to figure out for themselves whether and to what extent participation is worthwhile. There is no onus on nonparticipation.[15]

Overall, it cannot be too difficult to arrive at preferences on most matters, because everyone does it. Just as we consider our connections with those who advocate petitions as a quick way of determining whether we would feel comfortable in signing, so do people in general learn how to know what they ought to prefer without knowing much about it. People who do not pay much attention to politics or public policy can nevertheless develop preferences by getting them from Berelson, Lazarsfeld, and McPhee's (1954) well-known ("But," as James Stimson [1986] reminds us, "we keep forgetting it") two-step flow of communication from activists to less attentive citizens. Stimson (1986) shows that "mass perceptions track activist positions." His thesis is that this social connection "accounts for the riddle of inattentive electorates who seem to know much of what they need to know to make policy informed choices." Wholly in the spirit of cultural analysis, Stimson concludes "that many of the things that matter in political life . . . have very little to do with individual psychological processes. They are macro behaviors, such as mediated cognitions, that require for understanding a focus on 'between' rather than 'within' individual effects" (pp. 4, 19, 20). All I would add is that "between . . . individual effects" *become* "within individual effects."

"System" or "person" blame are dead giveaways. The slightest clue as to whether the authorities and the institutions vis-à-vis individuals are at fault helps people know whether they want to go along with egalitarian or hierarchical or market policies. Anyone who thinks that attribution of blame to "the system" or to individuals is not diagnostic should consult Table 2 from Verba and Orren, in which they address that very question to a variety of elites. The differences could hardly be greater (Wildavsky 1985b).

If it were necessary to go back to the cultural source each time a new preference is involved, building back up to the actual preference through some sort of chain of inference, many people could not manage the complexity; hence there would be far fewer preferences. Consequently, concerted political action would be a rarity. Near universal preference formation requires that preferences be inferred from all possible directions. Culture is the India rubber man of politics, for it permits pref-

Table 2. Poverty in the United States

Group	Fault of Poor	Fault of System
Business	57%	9%
Labor	15	56
Farm	52	19
Intellectuals	23	44
Media	21	50
Republicans	55	13
Democrats	5	68
Blacks	5	86
Feminists	9	76
Youth	16	61

Source: Verba and Orren 1985, p. 74.

erences to be formed from the slimmest clue. By knowing who or what is involved, the arena or institution of involvement, the subject or object of involvement, people know whether they are supposed to have preferences and what these preferences ought to be.

What is it that enables everyone to come up with reliable solutions to the problem of preference formation whenever it arises? The one source all human beings know something about is their social relations.

Cultures Constitute Our Political Selves

> Even when I carry out *scientific* work—an activity which I seldom conduct in association with other men—I perform a *social,* because *human* act. It is not only the material of my activity—like language itself which the thinker uses—which is given to me as a social product. *My own existence* is a social activity.
>
> —Karl Marx

The view of human life as suffused in social relations makes the study of institutions central to political science. To use Elkin's apt expression, "Values are thus 'in' politics, not above or outside it. Hannah Arendt makes the point when she comments that in political activity 'the end (telos) is not pursued but lies in the activity itself' " (1985, 17–18). Michael Oakeshott's (1962) insistence on good form and better manners, his "idioms of conduct," is based on the understanding that the purposes institutions create are expressed in their practices (Elkin 1985, 17–18). Elkin goes on to say that "the institutions are a way in which citizens experience each other and for different institutions the form of experience is different. . . . Political institutions constitute the citizenry in the sense of . . . giving it an organized existence" (pp. 16–17). Wolin (1986) defines democracy as I would a political culture: "Democracy involves more than participation in a political process. It is a way of constituting power" (p. 2). Similarly, Connell and Goot explain that "politics must be invoked not merely as the outcome of political socialization but a cause thereof as well" (Cook 1985).

On the level of ideas, a research program on political culture would seek to increase our understanding of how opposed visions of the good life are selected, sustained, altered, and rejected. As social scientists following Robert Merton and knowing, therefore, that unanticipated consequences are a staple of social life, we want to understand what else we choose when we choose our political cultures.

The Great Depression was a market phenomenon. The great holocaust was perpetrated by a hierarchy (the Nazi party) that tolerated no rivals. The second greatest holocaust was perpetrated by egalitarians (the Cambodian Khmer Rouge) (Jackson n.d.). Deadly visions as well as virtues are also rooted in our public lives. Appraising the consequences of living lives of hierarchical subordination or of the purely voluntary association of egalitarian liberation or of the self-regulation of individualistic cultures, at different times, on different continents, with different technologies, languages, and customs would be a remarkably productive research program. So would comparing cultures rather than countries or, put precisely, comparing countries by contrasting their combinations of cultures. Such a research program would enable us to test the general hypothesis that how people organize their institutions has a more powerful effect on their preferences than any rival explanation—wealth, technology, class, self-interest, tradition, you name it. The field of preference formation is open to all comers.

Notes

This essay is the presidential address presented on August 28, 1986 at the 82nd annual meeting of the American Political Science Association.

1. "Interest explanations are reason explanations. That is, when we explain an action by pointing to the interest that prompted, produced, or motivated it, we allude not to a human cause but to a *reason* or *ground* for acting" (Ball 1979, 199). Reasons justify our behavior to others.

2. See a perceptive paper by James March for a list of "the properties of tastes as they appear in standard prescriptive theories of choice." His list includes two properties of special interest: "Tastes are *relevant*. Normative theories of choice require that action be taken in terms of tastes." Yet, "tastes are *exogenous*. Normative theories of choice presume that tastes, by whatever process they may be created, are not themselves affected by the choices they control." As March observes, "each of these features of tastes seem inconsistent with observations of choice behavior among individuals and social institutions" (1978). Keith Hartley's paper "Exogenous Factors in Economic Theory" explains the general perspective, "Utility or preference functions are central to neoclassical economics and are assumed to be given" (1985, 470).

3. Michael Thompson and I are working on a book, *The Foundations of Cultural Theory*, that will attempt to demonstrate this proposition.

4. "Fatalistic attitudes are discernible in many Romanian literary creations, indeed even in folklore. The most famous Romanian folk ballad is 'Miorita,' or 'The Lamb.' It is the moving, beautiful story of a Moldavian shepherd whose fellow shepherds plot to kill him and steal his flock. Learning of the plan from his 'wonder lamb,' the young shepherd makes no move to keep it from being carried out. He serenely accepts his fate, comforted by the thought that he will be reunited with nature" (Shafir 1983, 405).

5. Michael Thompson argues in favor of the viability of his hermit category, a marketlike people who, however, seek subsistance rather than domination, to escape (a) from manipulating others and (b) from being manipulated themselves. I wish them luck (see Thompson 1982).

6. A test of cultural consistency is provided by what March and Harrison call "Postdecision Surprise." When things go badly, the excuses should fit the culture. The market-oriented should accept more personal responsibility than the members of a hierarchy; egalitarians should blame "the system" (see Harrison and March 1984).

7. Many more cultures can be conceived than can be lived in. As Robert Lane says, "Although for every act there is an implicit or explicit belief to justify the act, the reverse is not true; not every thought, fantasy, image, or argument is reflected in behavior, especially since thoughts often rehearse alternative lines of behavior. The world of behavior, therefore, is smaller than the world of thought; the two worlds are not isomorphic" (Lane 1973, 97).

8. Another common confusion is mixing up egalitarianism with exclusive hierarchies because they are both passionate and moralistic in defending their strong group boundaries. The fervor of the exclusive hierarchy comes from its simplicity: only a small number of prescriptions are applicable to the vast diversity of life forms. Deviance (and, hence, deviants) are excoriated. The difference is that whereas egalitarians find society at fault because the distances between people are too large, members of hierarchies believe that the moral spaces are too small. The lack of complexity in small hierarchies means that they are left without sufficient variety for the objects they wish to control. Therefore, they get rid of people who do not fit either by labeling them as deviants or treating them as moral trash, thus removing them from those who deserve to be taken into account in making decisions. The alternative is to co-opt more people by creating moral, and hence social, compartments for them. Using variety to

cope with complexity (as in Ashby's "Law of Requisite Variety"), however, greatly increases the size (the number and diversity of subunits) of the hierarchy. Once hierarchy is complex, as almost any but the most rudimentary government must be, its fervor declines because of the necessity of accommodating a broad range of values and adjusting relationships among more diverse life-styles.

9. For the reasons given, I disagree with the view that liberals are pro- and conservatives anti-change (see McCloskey and Chong 1985; and Robinson 1980).

10. The widespread belief among those who theorized about Jacksonian democracy in his time, a belief apparently shared by their supporters in the citizenry as well, was that equality of opportunity, meticulously followed, would lead to an approximation of equality of result. The operation of economic markets, unimpeded by the federal government, would eventually approximate real equality of condition as closely as innate differences in human ability permitted. At the very least, central government would not add artificial to natural inequality, thereby preserving representative government. Individuals would be allowed, indeed encouraged, to keep all gain that resulted from the unfettered use of their own talents. But everything artificial and unnatural, everything government imposed on man in his free state, such as charters, franchises, banks, and other monopolies, became anathema. It is this belief—not in equality undefined nor in just one kind of equality but in the *mutual reinforcement of opportunity and result*—that I think made the United States truly exceptional. Another way to describe U.S. exceptionalism is to say that liberty (i.e., individualism) is held to be compatible with equality (egalitarianism). Just as supporters of hierarchy understand that their organizations are likely to be rigid and egalitarians recognize that perfect equality is unattainable, so adherents of U.S. individualism understand that liberty can conflict with equality and vice versa. What they deny is that this conflict is immutable, and what they affirm is that their two cherished passions, liberty and equality, can reinforce one another (see Wildavsky 1986).

11. The impasse comes through in these two comments: 1) "There is mounting evidence that mass publics do not react in ideological terms. It seems equally true that much contemporary political conflict has an ideological dimension" (Barnes 1966, 513). 2) "Although the conceptual and methodological problems with ideological belief systems are serious enough for Bennett to have called for a moratorium on empirical research pending the development of better concepts and measures, a 'cottage industry' of comments and rebuttals continues to fill the journals with challenges to each and every piece of research" (Hamill, Lodge, and Blake 1985, 850).

12. Often distinctions are made between active and passive risk (active risk being more voluntary and controllable by the individual and passive risk less voluntary and perhaps uncontrollable) in order to justify why certain risks are more and other risks less acceptable. But the distinction is misleading. One might imagine a static social system whose values, including its rules of accountability, were petrified. The people who conferred meaning on objects must have lived long ago, no one having come along since with any changes to make. Classifications are clearly labeled and immobile. Then, and only then, might one allocate dangers according to those that are active and voluntary and, therefore, properly subject to governmental regulation or prohibition. Once social change enters the picture, however, the active-passive distinction is constantly redrawn. We now see that egalitarians consider the dangers stemming from technology (nuclear power or chemical carcinogens) as a passive risk, while they perceive the dangers stemming from casual contact with sufferers from acquired immune deficiency syndrome as an active risk. At the very same time, adherents of hierarchy view the dangers of technology as actively chosen. To say a danger is voluntary is tantamount to saying it is acceptable; involuntary dangers imposed on passive people, by contrast, are unacceptable. Classification and decision are one and the same. If the anger against institutions were comprehensive enough, suicides would be owed redress by the implacable institutions that drive them to their undeserved and involuntary end. Just as "we the people" are the ones who confer meaning on these distinctions, so we are also the ones who change these meanings.

13. The actual figures are 1) more say: U.S. citizens 64.5%, British 68.6%; 2) order: U.S. citizens 4.3%, British 2.1%; 3) freedom of speech scored a little over 25%; 4) rising prices were 4% or below.

14. *Oakland Tribune*, 10 August 1986.

15. This discussion of the conception of apathy as a part of cultural bias is congruent with Carole Pateman's view that "there is more than one way to *interpret* the norm of political efficacy and the other norms and values traditionally associated with democracy; there is more than one view on what 'really' constitutes responsiveness of leaders and so on, and these differences in interpretation also encompass divergent notions of what form[s] of democratic institutions actually embody, or give practical expression to those norms and values" (1971, p. 304).

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WHAT MOVES PUBLIC OPINION?

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Democratic theory must pay attention to what influences public opinion. In this study the content of network television news is shown to account for a high proportion of aggregate changes (from one survey to another) in U.S. citizens' policy preferences. Different news sources have different effects. News commentators (perhaps reflecting elite or national consensus or media biases) have a very strong positive impact, as do experts. Popular presidents tend to have positive effects, while unpopular presidents do not. In contrast, special interest groups tend to have a negative impact.

Public opinion is supposed by some to be the great engine of democracy, determining what governments do. Recent evidence has indicated that public opinion does in fact have substantial proximate effects upon policymaking in the United States (Erikson 1976; Monroe 1979; Page and Shapiro 1983a; Shapiro 1982; Weissberg 1976). The next question, however, is, What moves public opinion? What affects citizens' policy preferences?

The answer makes a great deal of difference. It would be premature to celebrate the triumph of democracy before knowing how and by whom the public is itself influenced. Does the public react directly to objective events, so that opinion is effectively autonomous? Do experts or enlightened political leaders educate the public with helpful new information? Or do demagogues or self-serving elites manipulate opinion with false or misleading propaganda? Which influences are most important: events, experts, politicians, interest groups? Do the mass media report relevant information accurately or inaccurately?

In this paper we make a start at answering such questions by investigating the impact upon public opinion of the statements and actions of certain actors as reported in the media.

Rational Citizens and the Mass Media

We consider citizens' preferences among alternative public policies to be primarily instrumental. That is, policies are judged in terms of expected costs and benefits for the individual and for his or her family, friends, favored groups, and the nation or world as a whole. But because there is great uncertainty about the effects of policies, the expected utility of a particular policy alternative depends critically upon beliefs about the state of nature, that is, beliefs about present and

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future facts and causal relationships (see McCubbins and Page 1984).

Thus new information that modifies relevant beliefs can change the expected utility of policies for citizens. This should occur if five conditions are met: if the information is (1) actually received, (2) understood, (3) clearly relevant to evaluating policies, (4) discrepant with past beliefs, and (5) credible. (For related views of attitude change, see Jaccard 1981; Zaller 1985.)

When these conditions are met to a sufficient extent, new information should alter an individual's preferences and choices among policies. Further, if the conditions are met in the same way for many individuals, there may be a change in collective public opinion that shows up in opinion polls. For example, if many citizens' policy preferences depend critically on the same belief (e.g., "We must spend more on national defense because the Russians are overtaking us") and if highly credible, well publicized new information challenges that belief (e.g., U.S. military spending is reported to rise sharply and a CIA study concludes that Soviet spending has changed little since 1976), then enthusiasm for increased military spending may drop.

Since most people have little reason to invest time or effort learning the ins and outs of alternative policies (Downs 1957), we would not expect new information ordinarily to produce large or quick changes in public opinion. Indeed the evidence indicates that aggregate public opinion about policy is usually quite stable (Page and Shapiro 1982).

By the same token, however, for whatever they do learn about politics, most people must rely heavily upon the cheapest and most accessible sources: newspapers, radio, and television, especially network TV news. When news in the media reaches large audiences and meets our five conditions for many individuals, we would expect public opinion to change.

Television news often meets the exposure condition. Most U.S. families own television sets, and most tune in to network news broadcasts from time to time. Viewers may wander in and out; they may eat or talk or be distracted by children; but every day millions of U.S. citizens catch at least a glimpse of the major stories on TV news. Others see the same stories in newspaper headlines or get the gist of the news from family and friends. Over a period of weeks and months many bits and pieces of information accumulate.

The conditions of comprehension and relevance, too, are often met. The media work hard to ensure that their audiences can understand. They shorten, sharpen, and simplify stories, and present pictures with strong visual impact so that a reasonably alert grade-schooler can get the point. Often stories bear directly upon beliefs central to the evaluation of public policies.

Credibility is a more complicated matter. Rational citizens must sometimes delegate the analysis or evaluation of information to like-minded, trusted agents (Downs 1957, 203–34). The media report the policy-relevant statements and actions of a wide variety of actors, from popular presidents and respected commentators, to discredited politicians or self-serving interest groups. News from such different *sources* is likely to have quite a range of salience and credibility, and therefore quite a range of impact on the public (see Hovland and Weiss 1951–52). The analysis of effects on opinion should allow for such variation.

News may also vary greatly in the extent to which it is or is not discrepant with past beliefs. If it closely resembles what has been communicated for many months or years, if it simply reinforces prevalent beliefs and opinions, we would not expect it to produce change. If, on the other hand, credible new information calls into question key beliefs and opinions held by many people, we would expect changes in public opinion. The extent of discrepancy with past news and past opinions must be taken into account.

We are, of course, aware of the curious notion that the contents of the mass media have only minimal effects (Chaffee 1975; Klapper 1960; Kraus and Davis 1976; McGuire 1985; but cf. Graber 1984; Noelle-Neumann 1973, 1980, 1984; Wagner 1983). This notion seems to have persisted despite findings of agenda-setting effects upon perceptions of what are important problems (Cook, Tyler, Goetz, Gordon, Protess, Leff, and Molotch 1983; Erbring, Goldenberg, and Miller 1980; Funkhauser 1973; Iyengar, Peters, and Kinder 1982; McCombs and Shaw 1972; MacKuen 1981, 1984).

We believe that the minimal effects idea is not correct with respect to policy preferences, either. It has probably escaped refutation because of the failure of researchers to examine collective opinion over substantial periods of time in natural settings and to distinguish among news sources. One-shot quasi-experimental studies (e.g., of presidential debates) understandably fail to find large, quick effects. Cross-sectional studies seek contrasts between media attenders and media "nonattenders" that hardly exist: nearly everyone is exposed either directly or indirectly to what the media broadcast (see Page, Shapiro, and Dempsey 1985a, 2–4). A more appropriate research design yields different results.

Data and Methods

Taking advantage of a unique data set in our possession, we have carried out a quasi-experimental study that overcomes several of the limitations of previous research. The design involved collecting data from many pairs of identically repeated policy preference questions that were asked of national survey samples of U.S. citizens; coding TV news content from broadcasts aired in between (and just before) each pair of surveys; and predicting or explaining variations in the extent and direction of opinion change by variations in media content.

Our design facilitated causal inferences and permitted comparison across types of issues and historical periods. The use of natural settings meant that all real world processes could come into play, including major events and actions, the interpretation of news by commentators and others, and the dissemination of information through two-step or multiple-step flows and social networks (cf. Katz and Lazarsfeld 1965). The examination of moderately long time periods (several weeks or months) allowed enough time for these natural processes to work and for us to observe even slow cumulative opinion changes. In addition, our measurement scheme permitted us to distinguish among different sources of news and to take into account the extent of news story relevance to policy questions, the degree of discrepancy between current and previous media content, and the credibility of news sources.

As part of our ongoing research project on public opinion and democracy, we have assembled a comprehensive collection of survey data on U.S. citizens' policy preferences. It includes the marginal frequencies of responses to thousands of different policy questions asked by various survey organizations since 1935. Among these data we have identified several hundred questions that were asked two or more times with identical (verbatim) wordings, by the same survey organization. (For a partial description, see Page and Shapiro 1982, 1983a.)

For the present research we selected 80 pairs of policy questions from the last 15 years (for which TV news data are readily available) that were repeated within moderate time intervals averaging about three months.

These 80 cases are not, strictly speaking, a sample from the universe of policy issues or poll questions but (with a small number of exceptions) constitute either a random sample of the available eligible survey questions and time points for a given survey organization or *all* the available cases from an organization. They are very diverse, covering many different kinds of foreign and defense (n = 32) and domestic (n = 48) policies. In nearly half the cases public opinion changed significantly ($p < .05$; 6 percentage points or more), and in a little more than half, it did not—nearly the same proportion as in our full data set of several hundred repeated items. A list of cases and a more detailed methodological discussion is available in Page, Shapiro, and Dempsey (1985a, b).

The dependent variable for each case is simply the level of public opinion at the time of the second survey (T2), that is, the percentage of the survey sample, excluding "don't know" and "no opinion" responses, that endorsed the most prominent (generally the first) policy alternative mentioned in the survey question. As will be seen, our method of using T2 level of opinion as the dependent variable and including first survey (T1) opinion as a predictor yields nearly identical estimates of media effects as does using a difference score—the magnitude and direction of opinion *change*—as the dependent variable.

For each of the 80 cases, we and our research assistants coded the daily television network news from one randomly selected network (in a few low-salience cases, *all* networks) each day, using the summaries found in the *Television News Index and Abstracts* of the Vanderbilt Television News Archive. These summaries, while rather brief and not intended for such purposes, were generally satisfactory in providing the fairly straightforward information we sought, especially since they were aggregated over several weeks or months. We coded all news stories that were at least minimally relevant to the wording of each opinion item, beginning two months before the T1 survey—in order to allow for lagged effects and for discrepancies or changes in media content—and continuing with every day up to T1 and through to the date of the T2 survey.

Being interested in the effects of particular actors or *sources*—particular providers of information, or Downsian "agents" of analysis and evaluation—whose rhetoric and actions are reported in the media, we distinguished among the original sources found in each news story. We used 10 exhaustive and mutually exclusive categories: the president; fellow partisans and members of his administration; members of the opposing party; interest groups and individuals not fitting clearly into any of the other categories; experts; network commentators or reporters themselves; friendly (or neutral) foreign nations or individuals; unfriendly foreign states or individuals; courts and judges; and objective conditions or events without clearly identifiable human actors (e.g., unemployment statistics, natural disasters, unattributed terrorist acts).

Our independent variables characterize *reported statements or actions by a specified source.* Each such *source story,* or "message," constitutes a unit of analysis in measuring aggregate media content over the time interval of a particular case. For each reported statement or action by a particular source—each source story—we coded the following: 1) its degree of *relevance* to the policy question (indirectly relevant, relevant, or highly relevant); 2) its *salience* in the broadcast (its inclusion in the first story or not, its proximity to the beginning of the broadcast, its duration in seconds; 3) the pro-con *direction* of intended impact of the reported statement or action in relation to the most prominent policy alternative mentioned in the opinion item; 4) the president's popularity (measured by the standard Gallup question) as an indication of his *credibility* as news source at the time of his statement or action; and 5) some judgments—not used in this paper—concerning the quality of the information conveyed, including its logic, factuality, and degree of truth or falsehood.

The most important part of the coding effort concerned the directional thrust of reported statements and actions in relation to each opinion question. Proceeding a little differently from the method of our earlier work on newspapers (Page and Shapiro 1983b, 1984), we measured directional thrust in terms of the intentions or advocated positions of the speakers or actors themselves. We took considerable care in training and supervising coders and in checking the reliability of their work. We prepared detailed written instructions and held frequent group discussions of coding rules and the treatment of problematic cases. All pro–con coding decisions, and those on other variables central to our analysis, were validated by a second coder and also by one of the present authors, who made the final coding decisions.[1] We masked the public opinion data so that coders would not be affected in any way by knowledge of whether or how policy preferences changed; we gave them only the exact wording of each opinion item and the time periods to be examined, not the responses to the questions.

As a result of these efforts we are confident that very high quality data were produced. It proved rather easy to code reported statements and actions on a five-point directional scale with categories "clearly pro," "probably pro," "uncertain or neutral," "probably con," and "clearly con" in relation to the main policy alternative outlined in each opinion question.

For each type of news source in each opinion case, we summed and averaged all the numerical values of pro–con codes (ranging from +2 to −2, with 0 for neutral) in order to compute measures of total and average directional thrust of the news from each source. The sums and averages of directional codes for television news content prior to T1 and between T1 and T2—for all messages coming from all sources combined and for messages coming separately from each distinct source—constitute our main independent variables. Most of our analysis is based on measures restricted to "relevant" or "highly relevant" source stories because we found that inclusion of less relevant source stories weakened the observed relationships.

Our principal mode of analysis was ordinary least squares regression analysis, in which we estimated the impact of each news source (or all sources taken together) along with opinion levels at T1, upon the level of public opinion at T2. We analyzed all cases together and also each of our two independently selected subsets of 40 cases, as well as subsets of cases involving different kinds of issues (e.g., foreign versus domestic policies), different time periods, and different levels of source credibility (popular versus unpopular presidents).

After testing hypotheses and exploring the aggregate data, we closely examined individual cases of public opinion change, scrutinizing media-reported statements and actions and the precise sequence of events. This served two purposes. First, it helped us with causal inference, shedding light on possibilities of spuriousness or reciprocal influence. Second, it enabled us to generate some new hypotheses about effects on opinion by certain sets of actors not clearly differentiated in our aggregate data.

Findings

We have argued that it is not appropriate to lump all media content together as if it came from a single source with a single level of credibility. It will be useful,

Table 1. Total TV News Content and Opinion Change

Variables	Coefficient
Opinion at T1	0.95[a] (23.25)
TV news content for two months pre-T1	-0.30[a] (-3.82)
TV news content between T1 and T2	0.11 (1.96)
Constant	0.34 (0.14)

R^2 = .88
adjusted R^2 = .88
n = 80

Note: Entries are unstandardized (*b*) coefficients from a regression of the percentage level of opinion at the time of the second (T2) survey on the level of opinion at T1 and the total media content variables (sums of pro–con scores from relevant stories) for all 80 cases. T values for *b*'s are given in parentheses.

[a]Significant at the .05 level or better by a two-tailed test.

however, to disregard our own advice for a moment and consider the effects upon public opinion of all TV news messages from all sources added together. In this way we can make clear the form of the relationship and especially the roles of pre-T1 news and of opinion at T1 in affecting the level of opinion at T2.

We regressed the level of opinion at T2 (that is, the percentage of respondents at T2 supporting the most prominent alternative offered in the survey question) upon (1) the level of opinion at T1, (2) the total sum of pro–con scores based on all relevant or highly relevant news stories from all sources combined in the two months before T1, and (3) the total pro–con sum in the T1–T2 period.[2] The results are displayed in Table 1.

The level of opinion at T1 is a very strong predictor of the level at T2; in fact by itself, it accounted for more than 85% of the variance in T2 opinion. That is to say, on the whole public opinion is quite stable over these periods of up to a few months. The average magnitude of opinion change is about 5 percentage points. There is a simple first-order autoregressive structure in levels of public opinion (b = .95). Thus regressions using the extent of opinion *change* rather than the level of T2 opinion as the dependent variable produce virtually the same coefficients for all the media content independent variables. Our results based on the level of opinion can equally well be interpreted as effects on opinion change.

Of more interest in Table 1 is the substantial negative effect that pre-T1 news had upon opinion at T2. A net sum of one "probably pro" story before T1 is associated with a drop of nearly one third (.30) of a percentage point in opinion at T2. This might seem puzzling at first, but it follows directly from our point that opinion change should depend upon a discrepancy or change in media content, given that opinion change is partly temporary.

If, for example, the TV news for several months before T1 was full of stories favorable toward a particular policy, so that opinion moved strongly in a pro direction before T1, and if the media were then utterly silent about the policy between T1 and T2, we would expect support for the policy to drop off as people forgot about or discounted the past news.

Thus opinion at T2 would be negatively related to media content before T1. If the discrepancy process worked in a particularly simple fashion (e.g., if all opinion changes were temporary and lasted exactly one period), we would find identical coefficients of opposite sign on corresponding pre-T1 and T1–T2 media variables, and we could use media content change scores to predict opinion change.

But things are not so simple. Part of the effect of media content is no doubt temporary, but part may last a long time, and some effects may be lagged or delayed. With our two-point time series we cannot precisely estimate lags or decay rates.[3] The problem is further complicated by the need to distinguish among news sources, some of which (e.g., commentaries, reports by experts) may have delayed effects and/or unusually slow decays. And the necessity of using T1–T2 periods of varying lengths, not always corresponding to the two-month pre-T1 period, unavoidably reduces the precision of estimating T1–T2 effects. Under these circumstances, our method of entering both pre-T1 and T1–T2 variables separately into regressions allows the maximum information to be extracted from the data.

We have elsewhere noted an interesting "falling off" effect in the polls that is closely related to the negative coefficient for pre-T1 news (Page and Shapiro 1983b; Page, Shapiro, and Dempsey 1984). It appears that pollsters frequently decide to ask survey questions about a particular policy alternative (often phrased as the first or "pro" alternative in the question) when that alternative is a lively topic in the media and public discussion. Thus an initial poll at T1 may reveal high public support for a newly publicized policy idea. Then those initial effects fade, and news coverage may tend to become more mixed, with doubts and opposition beginning to be heard. By the time of a second survey at T2, public support tends to drop a bit. We find a small negative opinion change (2.7 percentage points) on the average in our data set.

Another finding in Table 1 is the weak effect of T1–T2 news content. The estimated coefficient is positive but very small and not quite significant at the .05 level.[4] The logic of our analysis would seem to indicate that T1–T2 variables should have effects of opposite sign and roughly the same magnitude as corresponding pre-T1 variables. But we would not take this nonfinding very seriously. The effects of pre-T1 and T1–T2 media content variables are both estimated to be very small in Table 1 because of the failure to distinguish among different sources of news. If some sources have negative effects and some have positive effects and some have no effects at all, it is not surprising that a measure combining all of them together has little relation to opinion change.[5]

The importance of distinguishing among news sources becomes clearly apparent when we regress opinion at T2 on pre-T1 and T1–T2 news variables from the 10 distinct types of sources (separate sums of pro–con scores for relevant stories from each source). The results are reported in the first column of Table 2.

Taken as a whole, this regression accounts for the great preponderance—more than 90%—of the variance in opinion at T2. Of course much of this is attributable to the effects of opinion at T1, but a comparable analysis with opinion change as the dependent variable still accounts for a very substantial portion of the variance, about half of it (R^2 = .57; adjusted R^2 = .41). This is quite striking, given the inevitable presence of sampling error in the original surveys and the presumably imperfect media summaries and coding procedures.

Again pre-T1 news tends to have negative effects, that is, opposite to those of corresponding T1–T2 variables (see the top half of the first column of Table 2).

Table 2. Effects of TV News from Different Sources

Variables	Full Regression	Some Variables Deleted
Opinion at T1	0.97[a] (23.82)	0.97[a] (25.95)
Pre-T1 news		
President	-0.47[a] (-2.43)	-0.42[a] (-2.48)
Members of president's party	-0.07 (-0.32)	
Opposition party	-0.51[a] (-2.51)	-0.53[a] (-2.75)
Interest groups	-0.29 (-1.34)	-0.23 (-1.17)
Events	-0.53 (-0.99)	-0.44 (-0.90)
Commentary	2.16 (1.79)	1.87 (1.66)
Experts	-0.16 (-0.11)	0.00 (0.00)
Foreign—friendly, neutral	0.22 (0.34)	
Foreign—unfriendly	-0.19 (-0.37)	
Courts	1.37 (0.72)	1.77 (1.01)
News between T1 and T2		
President	0.30 (1.34)	0.23 (1.61)
Members of president's party	-0.09 (-0.73)	
Opposition party	0.44 (2.00)	0.46[a] (2.39)
Interest groups	-0.38 (-1.93)	-0.33[a] (-2.00)
Events	0.54 (1.27)	0.55 (1.52)
Commentary	4.34[a] (4.25)	4.17[a] (4.57)
Experts	3.37[a] (2.32)	2.85[a] (2.64)
Foreign—friendly, neutral	0.08 (0.14)	
Foreign—unfriendly	0.48 (0.99)	
Courts	-2.02[a] (-2.22)	-2.08[a] (-2.40)

Table 2 (continued)

Variables	Full Regression	Some Variables Deleted
Constant	-1.34	-1.11
	(-0.56)	(-0.49)
R^2 =	.94	.94
adjusted R^2 =	.91	.92
N =	80	80

Note: Entries are unstandardized (*b*) coefficients from regressions of opinion at T2 on opinion at T1 and the sums of the relevant pro–con news story scores from various sources, for all 80 cases. T values for *b*'s are given in parentheses.

[a]Significant at the .05 level or better by a two-tailed test.

Most important, however, news from different sources tends to have effects of different magnitudes and sometimes different directions. Source differences are apparent both among the pre-T1 variables and among the more readily interpretable T1–T2 variables, displayed in the bottom half of Table 2. We will focus on the latter,[6] treating pre-T1 variables as controls.

News commentary (from the anchorperson, reporters in the field, or special commentators) between the T1 and T2 surveys is estimated to have the most dramatic impact. A single "probably pro" commentary is associated with more than four percentage points of opinion change! This is a startling finding, one that we would hesitate to believe except that something similar has now appeared in three separate sets of cases we have analyzed. It was true of editorial columns in our earlier analysis of 56 two-point opinion series using the *New York Times* as our media source (Page and Shapiro 1983b), in the first 40 TV news cases we collected (Page, Shapiro, and Dempsey 1984), and in the 40 new TV cases, which we analyzed separately before doing all 80 cases together.

We are not convinced that commentators' remarks in and of themselves have such great potency, however. They may serve as indicators of elite or public consensus (Hallin 1984; McClosky and Zaller 1984; Noelle-Neumann 1973, 1980). Or the commentaries may—if in basic agreement with official network sentiment or the attitudes of reporters (perhaps providing cues for reporters)[7]—indicate slants or biases in media coverage that are transmitted to citizens in ways that supplement the statements of the commentators. These could include the selection of news sources and quotes, the choice of visual footage, the questions asked in interviews, camera angles, and so forth.

Certain other estimated effects on opinion are probably important even though some do not reach the .05 level of statistical significance according to a conservative two-tailed test.[8] Most notably—and clearly significantly—a single "probably pro" story about experts or research studies is estimated to produce about three percentage points of opinion change, a very substantial amount. Presidents are estimated to have a more modest impact of about three-tenths of a percentage point per "probably pro" story, and stories about opposition party statements and actions may also have a positive effect.

There are indications, on the other hand, that interest groups and perhaps the courts (in recent years) actually have negative effects. That is, when their statements and actions push in one direction (e.g., when corporations demand subsidies or a federal court orders school integration through busing) public opinion tends to move in the opposite direction. We are not certain about the negative effect of courts, however, because of the instability of coefficients across data sets.

Certain kinds of news appear on the average to have no direct effect at all upon opinion, or less impact than might be expected. The president's fellow partisans, when acting independently of the president himself, do not appreciably affect opinion. Events may move public opinion indirectly, but they do not speak strongly for themselves. They presumably have their effects mainly through the interpretations and reactions of other news sources. The same applies to statements and actions from foreign countries or individuals, whether friends or foes. U.S. citizens apparently do not listen to foreigners directly but only through interpretations by U.S. opinion leaders.

The marked distinctions among types of news fits well with our idea that information from different sources has different degrees of credibility. It is quite plausible, for example, that the public tends to place considerable trust in the positions taken by network commentators and (ostensibly) nonpartisan experts. Some other sources may be considered irrelevant. Still others, like certain interest groups that presumably pursue narrowly selfish aims, may serve as negative reference points on public issues (see Schattschneider 1960, 52–53). Similarly, the federal courts may have served as negative referents in the 1970s and the early 1980s because of their unpopular actions on such issues as busing and capital punishment. In any case, it is clearly important to distinguish among sources of news.[9]

In the second column of Table 2, we report the results of a modified regression analysis in which we dropped some variables (party of the president, foreign friends and foes) that had small and unreliably estimated coefficients in the previous regression. The results are much the same except that most of the coefficients are more stable and the effects of interest groups and opposition party appear statistically significant even by the conservative two-tailed test. News commentary and experts remain the most powerful sources of opinion change.

An interesting finding is that while most of the news variables have pre-T1 coefficients opposite in sign to those for T1–T2—consistently with the discrepancy and temporary effect hypothesis—commentary does not. Commentaries may in fact have lagged positive effects that take time to operate as the commentators' views (or the consensuses or biases they reflect) diffuse through the political system. By the same token, part of the negative effect of interest groups may be a lagged one as well.

For one news source, namely presidents of the United States, we were able to explore the credibility issue directly. We consider a president's popularity—that is, the percentage of U.S. citizens who approve his "handling of his job" according to the Gallup poll—to be a good indicator of the general level of trust and confidence in a particular president. When a president is popular we would expect people to put more faith in what he says and does and to be more prone to change their opinions accordingly. In order to test this hypothesis we partitioned our data into two subsets of cases: one in which, at the time of the T1 survey, the president had an approval rating of 50% or higher (n = 35); and the other in which approval was less than 50% (a larger n = 45 in the unhappy period studied). We performed the analysis of TV news impact separately for each of these subsets of cases, with the results displayed in Table 3.

When presidents are popular, they tend (though the estimate falls short of statistical significance) to have a small positive effect on public opinion. Each "probably

Table 3. Presidential Popularity and TV News Effects on Opinion

Variables	When Presidents Were Popular	When Presidents Were Unpopular
Opinion at T1	0.89[a] (10.78)	1.00[a] (16.77)
Pre-T1 news		
President	-0.64 (-0.97)	-0.66 (-1.50)
Members of president's party	-0.19 (-0.32)	-0.50 (-0.77)
Opposition party	-0.69 (-1.14)	-0.71 (-1.22)
Interest groups	-1.19 (-1.54)	-0.52 (-1.28)
Events	-3.07 (-1.24)	0.63 (0.70)
Commentary	1.00 (0.51)	1.85 (0.57)
Experts	-3.64 (-1.35)	-4.44 (-1.09)
Foreign—friendly, neutral	0.91 (0.75)	1.86 (1.10)
Foreign—unfriendly	-0.61 (-0.71)	-15.55 (-0.96)
Courts	-2.52 (-0.37)	2.19 (0.76)
News between T1 and T2		
President	0.58 (1.55)	0.05 (0.05)
Members of president's party	-0.41 (-1.79)	0.40 (0.82)
Opposition party	0.84[a] (2.18)	0.23 (0.50)
Interest groups	-0.15 (-0.44)	-0.46 (-0.41)
Events	0.53 (0.53)	1.15 (0.60)
Commentary	2.51 (1.56)	6.16 (1.74)
Experts	7.86 (1.46)	6.89[a] (2.39)
Foreign—friendly, neutral	-2.57 (-1.94)	-0.51 (-0.46)
Foreign—unfriendly	-1.04 (-1.07)	3.78 (0.73)
Courts	0.52 (0.26)	-0.91 (-0.56)

Table 3 (continued)

Variables	When Presidents Were Popular	When Presidents Were Unpopular
Intercept	4.82 (1.02)	-3.72 (-1.01)
R^2 =	.97	.95
adjusted R^2 =	.93	.91
N =	35	45

Note: Entries are unstandardized (*b*) coefficients from regressions of opinion at T2 on opinion at T1 and the sums of relevant news story pro-con scores from various sources. "Popular" presidents had Gallup poll approval ratings of 50% or more at T1; unpopular presidents had ratings under 50%. T values for *b*'s are given in parentheses.

[a]Significant at the .05 level or better by a two-tailed test.

pro" statement or action is estimated to produce more than half a percentage point of opinion change. Part of the effect is undoubtedly temporary and part reciprocal. The impact presumably could not be multiplied indefinitely by talkative presidents because of potential saturation and overexposure and the reporters' and editors' desires for fresh topics to cover. Still, this constitutes some evidence that a popular president does indeed stand at a "bully pulpit." On an issue of great importance to him he can hammer away with repeated speeches and statements and can reasonably expect to achieve a 5 or 10 percentage point change in public opinion over the course of several months (see Page and Shapiro 1984).

Unpopular presidents, in contrast, apparently have no positive effect on opinion at all. They may try—like Glendower in *Henry IV*—to call spirits from the vasty deep, but none will come.[10]

There are some indications that the effects of other news sources interact with presidential popularity. While the full set of possible first-order interactions is too complicated to model with confidence given the number of cases we have, these separate popular and unpopular president regressions indicate that commentaries may have their strongest effects when presidents are unpopular. Perhaps news commentators substitute for a respected leader, challenging the one that is out of favor. In addition, administration officials and the president's fellow partisans in Congress and elsewhere, when acting independently of a popular president, appear to have a slightly negative impact on opinion, whereas they may have positive effects when presidents are unpopular. The opposition party, rather strangely, seems especially potent when presidents are popular. In short, there may be some substantial differences in the dynamics of opinion change depending upon whether the president in office at a particular time is popular or not.

Discussion

Our examination of a number of specific cases of opinion change has bolstered our general confidence in the aggregate findings. It has also illuminated certain issues of causal inference and has generated some new hypotheses about further differentiations among different actors or sources of news. Because we have reported on the cases in detail elsewhere (Page, Shapiro, and Dempsey 1985a, b), we will mention only a few important points about particular news sources.

News Commentary

The most dramatic finding in Table 2 is the strong estimated impact of news commentary. Our examination of specific cases provides a number of instances in which the statements of news commentators and reporters clearly parallel opinion change. Examples include Howard K. Smith's praise for Nixon's policies and his criticism of calls for unilateral withdrawal from Vietnam in 1969; various newsmen's support for continued slow withdrawal from Vietnam during 1969-70; commentary favoring conservation and increased production rather than stopping military aid to Israel in order to get cheap oil during 1974-75; Smith's and others' support for more attention to the Arabs during 1974-75 and during 1977-78; Eric Severeid's, David Brinkley's, and Smith's advocacy of campaign contribution limits in 1973; Brinkley's and Smith's backing of stricter wage and price controls during 1972-73; John Chancellor's editorializing on the importance of fighting unemployment (versus inflation) in 1976; Smith's support for federal work projects in 1976; and commentaries in the spring of 1981 that Reagan's proposed tax cuts would benefit the wealthy.

Our regression estimate of very large commentary effects, controlling for all other news sources, indicates that something substantial was going on. We cannot entirely rule out reciprocal effects of public opinion upon audience-seeking reporters and media, but in many cases, the timing of news commentary shortly after T1 polls indicates some kind of genuine influence upon opinion at T2.

The exact nature of that influence is harder to judge, however. We would not claim that individual news commentators like Howard K. Smith—for all the esteem in which they are held—are, in themselves, the biggest sources of opinion change (but cf. Freeman, Weeks, and Wertheimer 1955). We do not believe that Walter Cronkite single-handedly ended the Vietnam War with his famous soul-searching broadcast in 1968.

Instead, the commentary we have examined may reflect the positions of many journalists or other elites who communicate through additional channels besides TV news or even a widespread elite consensus in the country (see McClosky and Zaller 1984). Or commentators' positions may be indicators of network biases, including subtle influences of reporters and editors upon the selection of news sources and upon the ways in which stories are filmed and reported. Or, again, commentators and other sources with whom they agree may (correctly or not) be perceived by the public as reflecting a climate of opinion or an emerging national consensus on an issue, which may weigh heavily with citizens as they form their own opinions (see Lippmann 1922; Noelle-Neumann 1973). With our present data, we cannot distinguish among these possibilities. But news commentators either constitute or stand for major influences on public opinion.

Experts

According to our estimates in Table 2, those we have categorized as "experts" have quite a substantial impact on public opinion. Their credibility may be high because of their actual or portrayed experience and expertise and nonpartisan status. It is not unreasonable for members of the public to give great weight to experts' statements and positions, particularly when complex technical questions affect the merits of policy alternatives.

The existence of a reciprocal process, influence by public opinion upon experts, cannot be ruled out (particularly to the extent that the audience-seeking media decide who is an expert based on the popularity of his or her policy views), but it is probably limited in the short run because experts do not face immediate electoral pressures—that is, public attitudes may ultimately influence who are considered experts and what their basic values are, but once established, experts are less likely than presidents or other elected officials to bend quickly with the winds of opinion.

One striking example of the influence of expert opinion as reported in the media concerns the Senate vote on the SALT II arms limitation treaty. Public support for the treaty dropped 5.5% from February to March 1979 and 19% from June to November. During both periods many retired generals and arms experts spoke out or testified against the treaty, citing difficulties of verification and an allegedly unequal balance of forces favoring the Soviets.

Experts seem also to have been important in building support for the 1981 AWACs sale to Saudi Arabia; in increasing skepticism about Reagan's tax cuts between May and June 1981; in cooling off enthusiasm for a tax on large cars in 1974; and in encouraging support for public financing of political campaigns (1973) and for banning handguns (1981).

We cannot tell from the present data how accurate or inaccurate expert testimony tends to be. There is reason to believe, for example, that the Russian scare of the late 1970s and early 1980s was greatly overblown (Halloran and Gelb 1983; Sanders 1983). Nor can we say much about the possibility that interest groups are important in funding and publicizing favorable expert studies (Saloma 1984) or that the media may be biased in choosing which experts to feature (perhaps favoring, for example, the political tides of the day). Such matters are quite important for any conclusive assessment of the role of the public in democracy, and we plan to pursue them further.

Presidents

As we have seen, public opinion tends to shift somewhat in the direction supported by a popular president. Our single equation regression analysis, however, cannot by itself exclude the possibility of reverse or reciprocal influence. Rather than leading the public, presidents may sometimes take positions (or make policy) in *response* to public preferences or in anticipation of future changes in public opinion (see Page and Shapiro 1983a). Popular presidents may be more apt than unpopular ones to try this and to succeed at it, hence, perhaps, their popularity in the first place (which is presumably augmented by taking popular stands).[11] Lacking continuous survey data between T1 and T2, we cannot be sure that unmeasured opinion did not change before some of the T1–T2 news reports that we have taken as causally prior. Nor, of course, can we be sure that presidents did not anticipate opinion changes.

In this situation our scrutiny of specific cases has been helpful. It has not ruled out causal complexities. On the contrary, we are convinced that the relationship between presidents and public opinion is reciprocal, with each influencing the other (Page and Petracca 1983). But numerous cases support the inference that popular presidents' actions and statements reported in the media do affect public opinion. These include President Nixon's persistent opposition to accelerating U.S. troop withdrawals from Vietnam during 1969, 1970, and 1971; Reagan's 1981 argument for AWACS airplane sales to Saudi Arabia; Carter's 1977–78 increased attention to Arab countries; Carter's early 1980 movement (during a temporary peak in popularity) toward toughness in the Iranian hostage crisis; Reagan's 1982 bellicose posturing toward the Soviet Union; Ford's 1974–75 defense of military spending; Ford's 1976 and Carter's 1980 advocacies of cuts in domestic spending; and, perhaps, Nixon's 1972–73 support for wage and price controls.

On the other hand, as our regression results showed, unpopular presidents do not have much success at opinion leadership. In a number of cases unpopular presidents made serious efforts to advocate policies but failed to persuade the public. This was true of Ford's attempts to increase military spending in 1976 and his resistance to jobs programs and health and education spending in the same year. Jimmy Carter in early 1979, with his popularity at 43% approval and falling, failed to rally support for SALT II. Carter was also unsuccessful at gaining significant ground on gasoline rationing, the military draft, or the Equal Rights Amendment in 1979 and 1980. Even Ronald Reagan, when near a low point of popularity (44%) in mid-1982, failed to move opinion toward more approval of a school prayer amendment to the Constitution. Because this distinction between popular and unpopular presidents emerged clearly in our previous analysis of newspaper data (Page and Shapiro 1984), we are inclined to believe that it is real (though modest in magnitude) even though the popular president effect does not quite reach statistical significance in Table 3.

Interest Groups

Our regression analysis indicated that groups and individuals representing various special interests, taken together, tend to have a negative effect on public opinion. Our examination of the cases supports this point but also suggests that certain kinds of groups may have positive effects while others have negative impact.

We found many cases (more than 20) in which public opinion unequivocally moved *away* from positions advocated by groups and individuals representing special interests. In some cases the groups may have belatedly spoken up after public opinion had already started moving against their positions, producing a spurious negative relationship. But in many instances they seem actually to have antagonized the public and created a genuine adverse effect.

Such cases include Vietnam War protestors from 1969 to 1970, protestors against draft registration in 1980, and perhaps the nuclear freeze movement in 1982. U.S. citizens have a long history of opposition to demonstrators and protestors, even peaceful ones, and apparently tend not to accept them as credible or legitimate sources of opinion leadership.[12]

In general, the public apparently tends to be uninfluenced (or negatively influenced) by the positions of groups whose interests are perceived to be selfish or narrow, while it responds more favorably to groups and individuals thought to be concerned with broadly defined public interests. The best examples of the latter in our data are environmental groups and perhaps also general "public interest" groups like Common Cause.

From 1973 to 1974, for example, support for leasing federal land to oil companies declined as TV news reported conservationists challenging the positions of the profit-seeking and presumably less credible oil companies. During the same period, support for a freeze on gasoline, heating, and power prices increased a bit despite opposition by gas station owners and oil companies.

Not only business corporations, but also some mass membership groups representing blacks, women, the poor, Jews, and organized labor seem to have been held in disrepute[13] and to have had null or negative effects on opinion about issues of direct concern to them, including social welfare policies and some Middle East issues.

Events, Foreign Countries, and Other Sources

The fact that our regression analysis showed some types of news sources to have, on the average, no clearly positive or negative effects upon public opinion, does not mean that such sources never have effects. As the example of interest groups suggests, a negligible net effect might conceal offsetting impacts by particular subclasses of sources under particular conditions. If it were feasible to subdivide our 10 source categories further, such effects would presumably be revealed by the statistical analysis.

Among the presidents' fellow partisans, for example, it might be useful to distinguish administration officials from congressional leaders, who may have more independence and a different impact upon public opinion. In the opposition party, too, key congressional leaders and media stars may be more influential than the rank and file.

For foreign actors it would perhaps be useful to distinguish statements (e.g., verbal threats) from actions or policies (e.g., military attacks). It is not easy to code either one so as to correspond to likely opinion change. Certain prominent foreign actors (e.g., Winston Churchill or the Pope) may merit special treatment, and our classification of foreign sources as friendly or unfriendly should perhaps be expanded to allow for finer distinctions.

We initially expected that objective events not attributable to particular actors—anonymous terrorist actions, changes in the rates of unemployment and inflation, natural disasters, and the like—might have substantial effects on public opinion. Contrary to this expectation, our aggregate analysis revealed little or no net direct effect of events. Examination of specific cases indicated that certain kinds of events probably do have appreciable impacts, especially changes in the consumer price index or unemployment rates, which can be felt directly by workers and consumers in their daily lives. Finer distinctions among types of events and allowance for variations in magnitude would permit quantitative estimates of such effects. We have come to believe, however, that much of the impact of objective events is indirect, mediated by U.S. political leaders and commentators and experts in ways that we have not yet fully untangled.[14] Events—like statements and actions from foreign countries—seldom speak directly and unambiguously to the public; rather they affect public opinion mostly through the interpretations and reactions of U.S. elites.

Conclusion

We believe we have identified the main influences on short-term and medium-term opinion change.

Our analysis does not offer a full account of certain glacial, long-term shifts in public opinion that reflect major social, technological, and demographic changes such as rising educational levels, cohort replacement, racial migration, or alterations in the family or the workplace. The decades-long transformations in public attitudes about civil liberties, civil rights, abortion, and other matters surely rest (at least in an ultimate causal sense) upon such social changes.[15] If news reports play a part in such major opinion shifts, they may do so mainly as transmitters of more fundamental forces.

Within the realm of short- and medium-term effects, however, we have had striking success at finding out what moves public opinion. Our TV news variables, together with opinion at the time of an initial survey, account for well over 90% of the variance in public opinion at the time of a second survey. The news variables alone account for nearly half the variance in opinion change.

This success is especially remarkable because of the many possible sources of error that might be expected to reduce our explanatory power: sampling and measurement error in the original opinion surveys; imprecision in the published news summaries and in our coding scheme; the varying lengths of T1–T2 periods and our inability precisely to model lagged effects or decay rates; and the lack of provision for differential audience receptivity or different population subgroup effects, not to mention our ignoring of possible opinion influences not reflected in TV news. Improvements in methodology would presumably reduce random error and strengthen the findings still further. But the present analysis already accounts for the bulk of observed change in public opinion concerning policy choices.

The processes of opinion change are not simple. In order to account for changes between two opinion surveys, for example, it is essential to examine media content before the first survey. *Discrepancies* between current news and prior news (or prior opinion) are important. Part of the media impact is temporary so that there is a tendency for opinion in the T1–T2 period to drift back, to move in a direction opposite to the thrust of the media content prior to T1.

Moreover, it is important to distinguish among news *sources* rather than aggregating all media content together. The effects of news from different sources vary widely.

Among the sources we examined, the estimated impact of news commentary is strongest of all, on a per-story basis, though such messages are aired less frequently than those from other sources. The causal status of this finding, however, is uncertain. Commentary may be an indicator of broader influences, such as media bias in the selection and presentation of other news, of consensus among the U.S. media or elites generally, or of a perceived public consensus.

Experts, those perceived as having experience and technical knowledge and nonpartisan credibility, also have very sizable effects. A policy alternative that experts testify is ineffective or unworkable tends to lose public favor; an alternative hailed as efficient or necessary tends to gain favor.

We found that messages communicated through the media from or about popular presidents tend to have positive effects on opinion. Presidents respond to public desires, but they can also lead public opinion (see Page and Shapiro 1984). Active presidential effort can be expected to yield a 5- or 10-percentage point change in opinion over the course of a few months.

News commentators, experts, and popular presidents have in common a high level of credibility, which we believe is crucial to their influence on the public. Rational citizens accept information and analysis only from those they trust. In contrast, news sources with low credibility, such as unpopular presidents or groups perceived to represent narrow interests, generally have no effect, or even a negative impact, on public opinion.

Some of these findings might be thought to be limited to the recent period we studied, in which the public has relied heavily on TV and is better educated and more attentive to politics than U.S. citizens in the past. Our confidence in the generality of the findings, however, is bolstered by their consistency with our previous analysis (using newspaper stories) of opinion change from 1935 onward (see Page and Shapiro 1983b, 1984). This similarity also reinforces the observation that the national news media in the U.S. are very much of a piece. They all tend to report the same kinds of messages concerning public policy, from the same sources. This can be attributed to the norms and incentives—and the organizational and market structure—of the news industry and especially to the pervasiveness of the wire services (see Epstein 1973; Gans 1980; Roshco 1975). In this respect the contents of one medium is a good indicator of the content of many media.

In terms of our concerns about democratic theory, it is interesting to observe that relatively neutral information providers like experts and news commentators apparently have more positive effects (at least direct effects) than do self-serving interest groups. It is also interesting that popular presidents, who presumably tend to embody the values and goals of the public, are more able than unpopular ones to influence opinions about policy. These findings suggest that objective information may play a significant part in opinion formation and change and that certain of the more blatant efforts to manipulate opinion are not successful.

On the other hand, unobtrusive indirect effects by special interests—through influences on experts and commentators, for example—may be more dangerous than would be a direct clash of interests in full public view. Clearly there is much more to be learned before we can be confident about the fundamental sources of influence on public opinion. The same is true of judging the quality of information received by the public.

In order to judge to what extent the public benefits from constructive political leadership and education and to what extent it suffers from deception and manipulation, we need to examine the truth or falsehood, the logic or illogic, of the statements and actions of those who succeed at gaining the public's trust (see Bennett 1983; Edelman 1964; Miliband 1969; Wise 1973; contrast Braestrup 1983; Robinson 1976; Rothman 1979). This applies to the sources whose messages are conveyed through the media and to the media themselves. There is much to learn about whether various sources lie or mislead or tell the truth; about how accurately or inaccurately the media report what the sources say and do; and about the causes of any systematic distortions or biases in the selection and reporting of policy-related news.

Notes

All three authors are associated with NORC (formerly National Opinion Research Center), Chicago. A longer version of this paper was presented at the 1985 annual meeting of the American Political Science Association, New Orleans. We have benefitted greatly from the suggestions and comments of Tom Ferguson, Alex Hicks, Henry Brady, Michael MacKuen, Robert Erikson, David Fan, John Zaller, Eleanor Singer, Herbert Gans, Phil Davison, Mathew McCubbins, John Ferejohn, Roger Noll, Barbara Geddes, Gavan Duffy, and especially Garth Taylor. Harpreet Mahajan assisted in preparing the manuscript. We thank the National Science Foundation for research support under Grant No. SES83-08767; the responsibility for analysis and interpretation is our own.

1. This was done by Dempsey for all cases and by Shapiro for some early cases. Shapiro also checked the coding and analyzed the written summaries for several detailed case studies. Any disagreements about coding were resolved through meetings and discussion. Some reliability analysis was done, with Dempsey and Shapiro coding cases independently. Their intercoder reliability coefficients for the variables coded were in the .7 and .8 range. For the all-important pro-con codes, the two authors never disagreed by more than one unit on the 5-point scale.

Coding, verifying, and keypunching the data for the 80 cases took an immense effort. More than 10,000 hours were spent preparing the case level, aggregated data file based on 10,950 source stories (messages or data lines). We are grateful to the following research assistants for their diligent work: Amy Richmond, Karl Mueller, Mandy Kwock, Sasha Heid, Joe Torres, Peter McCarthy, Marianne Eismann, Chris Hill, Dan Sakura, Susan Rosenberg, Kathy Szydagis, Francis Kwakwa, John Kendzior, Mennette Masser, Jim Martin, Lance Selfa, Bill Sullivan, Wayne Arney, Ion Motkin, and Ellen Seebacher.

2. We performed the analysis for *average* media content (mean pro-con codes) as well as for sums. Averages do not account nearly as well for opinion change, however; the volume as well as the directional thrust of news is important. Hence we report the results for sums only.

3. For sophisticated efforts to do so using longer time series see Erbring (1975) and MacKuen (1981, 1983) on agenda setting and Fan (1984, 1985a-c) on a few policy preference items.

4. Given our unusual sampling scheme, the reported significance levels may be taken as referring to a hypothetical universe of similar cases (weighted by survey organization) or as informal indicators of substantive significance. They also provide some protection against erroneous inferences due to random measurement errors in the survey data and media coding. As we will note below, however, the two-tailed t test is quite conservative—probably too conservative—given our past work with different data sets.

5. The T1-T2 coefficient is probably reduced

more than the pre-T1 because of measurement error due to the variation in T1–T2 period length.

6. The interpretation of pre-T1 coefficients is not entirely straightforward. By the logic of a partial adjustment model, they could be considered estimates (with reversed signs) of temporary effects: that is, effects that appear in the T1 opinion measurement but disappear by T2. This interpretation is complicated, however, by the apparent presence of some lagged positive effects of pre-T1 media content on opinion at T2. Because we cannot identify lags or decay rates with our short time series, we cannot sort out these complexities. We have, therefore, chosen to focus our interpretation on the T1–T2 variables (not attempting to distinguish what part of their effects lasts how long), treating pre-T1 factors as controls. We should once again note, however, that the uneven T1–T2 periods introduce measurement error that may depress T1–T2 estimates relative to pre-T1 estimates.

7. We are indebted to Tom Ferguson for this point.

8. Our previous data analyses gave us a priori expectations that would justify a more liberal one-tailed test. By the same token, a macrotest involving all three data sets would indicate that repeated estimates of small coefficients with the same sign were highly unlikely to arise by chance.

9. Fan (1985c) reports substantial media effects upon opinion about defense spending despite all news sources being combined. It is not clear whether special characteristics of the defense issue produce this result or whether it is applicable to other issues.

10. Our thanks to Jim Davis for improving our recollections of Shakespeare.

11. Mark Petracca and Jeffrey Tulis, among others, have emphasized this point.

12. This is not to deny that protestors may indirectly play a role in moving the public and policy makers, for example, against the Vietnam War (see Burstein 1979; Burstein and Freudenberg 1978). Protestors raised the domestic costs of the war and expanded its visibility, and, in the long run, many of their positions met the test of reality and were accepted as correct.

13. Ideally one would gather extensive survey data bearing directly upon the credibility and popularity of such groups at various times and use it explicitly in the analysis. The limited data available support our assessment of the relatively low level of public esteem in recent years for organized labor, special economic interests, and certain liberal groups, as well as the greater credibility of environmental and public interest groups (see Lipset and Schneider 1983).

14. Before we controlled for other sources, events (the T1–T2 pro-con sum) were significantly related to opinion change: a b coefficient of 1.11 ($t = 2.56$, $p < .05$), while controlling for pre-T1 events ($b = -0.80$, $t = -1.44$, n.s.). This suggests that exogenous events may have large indirect effects through other sources, effects that vanish when those sources are included in the regression as in Table 2.

15. See, for example, Davis (1975). Page and Shapiro are currently completing a monograph on changes in U.S. public opinion since 1935, which considers long-term social changes as well as media-reported influences.

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RESIDENTIAL MOBILITY AND VOTER TURNOUT

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We examine the characteristics of a largely ignored low-turnout group—people who have recently moved. We find that neither demographic nor attitudinal attributes explain their lower turnout. Instead, the requirement that citizens must register anew after each change in residence constitutes the key stumbling block in the trip to the polls. Since nearly one-third of the nation moves every two years, moving has a large impact on national turnout rates. We offer a proposal to reduce the effect of residential mobility on turnout and estimate that turnout would increase by nine percentage points if the impact of moving could be removed. The partisan consequences of such a change would be marginal.

Fifty-three percent of the voting age population went to the polls in the 1980 general election. In the previous two years, one-third of all adults moved. We investigate the connection between these two facts and show that the turnout rate in the United States reflects in part the low turnout of those who have moved. The relationship of mobility to turnout is particularly interesting because movers are nearly as interested in politics and as fully convinced that they have a say in what the government does as people who have stayed put. Drawing on our research on mobility, we offer a proposal we believe would bring more people to the polls. We find that turnout would increase by nine percentage points if the effect of moving were removed. Finally, we assess the political consequences of reducing the deleterious impact of moving on turnout.

For most U.S. citizens the trip to the polls is a two-step process. First one must register; then and only then can one vote. Election day is not likely to slip by unnoticed. Lavishly publicized, it is the climax of a long, hard-fought competition, the result of which is widely awaited. Everyone except an absentee votes on the same day; the act itself is a shared experience. None of this is true of registration. Deadlines, requirements, and procedures are shrouded in obscurity. In most states registration is closed in the month before election day, when the excitement and publicity of the campaign reach a peak. It should not be surprising that 31% of all U.S. citizens of voting age failed to register for the 1980 general election nor that this country's dismal showing in international comparisons of turnout is due in large measure to our registration system, in which the individual, not the government, bears the responsibility for establishing one's eligibility to vote.[1]

The registration barrier does not impede all sections of society equally, a fact

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only partially recognized in the numerous voter registration campaigns aimed at various groups believed to need help in dealing with the barrier. One obvious category is newly eligible young people facing their first opportunity to vote and not familiar with registration procedures. A second and larger group is people not sufficiently interested or skilled to surmount the bureaucratic hurdle of registration. The identifying characteristic of these people is low education; they are usually described as "the poor and minorities."

A third group whose voting is impeded by the registration requirement consists of those who have moved recently. Whenever one moves, whether it is down the street, to a nearby town, or across the country, it is necessary to register anew. As a result, movers (those who moved within two years of election day) had a turnout rate in 1980 just as low as young people or the uneducated. Moreover, the movers greatly outnumber these other groups. In 1980, movers represented 29% of voting age citizens, while people with incomes below the poverty line, blacks, and those just old enough to vote in a presidential election were respectively 22%, 11%, and 10% of the electorate. Although the residentially mobile are the largest light-voting group, they have received little attention from scholars. Before analyzing movers, we will examine more closely our basic assertion—that registration is indeed a major impediment to voting.

The Registration Obstacle

The decentralization of election administration, combined with the primitive level of record keeping in many jurisdictions, creates something of a mine field for researchers attempting to establish how many people are registered in order to compute how many of those who are registered go to the polls. Each side of the percentage computation is questionable; each deficiency contributes to underestimating turnout. The numerator of this computation—the number of votes cast—is somewhat less than the number of all voters because it excludes spoiled ballots, people who go to the polls but do not vote for a presidential candidate, and those whose write-in votes are not counted. The Census Bureau estimated that counting all such ballots would have increased the official turnout figure by about two percentage points in 1976 and one point in 1980 (U.S. Bureau of the Census 1982, 7).

The greater difficulty is the denominator, the number who are registered to vote. It is important to understand that state registration records are not a satisfactory source because there are no widely employed methods of removing in a timely fashion the names of people who have died or moved. Most states do purge the names of those who have not voted for a prescribed length of time, most commonly four years. But some people will have died and a great many will have moved before their time is up, thus any state's records will always be out of date. Moreover, at least 20% of the population lives in states with no purging requirement at all.

The inadequacy of state records may be judged by a recent episode in Rhode Island, a state that purges the names of those who have not voted for five years. In the fall of 1983, informational pamphlets on the impending statewide election were routinely mailed to the state's 530,000 registrants. Because these are mailed at the bulk rate, in the past all undelivered ones had been destroyed by the post office. But in 1983 the Secretary of State, Susan L. Farmer, arranged for the post office to return undelivered pamphlets to her office. More than 100,000 came back because the addresses were not at their official voting addresses. In California, the Secretary of State's office estimated in 1986 that 9% of all registrations are "deadwood"—people who have died or moved. Because all state registration rolls are inflated in this way, voter turnout cannot be accurately measured by dividing the number of votes cast by the number of persons registered.

Surveys do not present this problem and are a more satisfactory source of data for our purposes. Of those available, we have most confidence in the Vote Validation Study conducted as part of the University of Michigan Center for Political Studies 1980 National Election Study (NES), in which interviewers visited county offices to establish through direct inspection of the records whether each respondent had registered and voted (see Appendix). The major drawback of this source is the somewhat "upscale" character of the Michigan sample, which underrepresents poor people, particularly uneducated young men. (For a comparison of data sources used in the study of turnout, see the Appendix.) Doubtless this is why the vote-validated Michigan NES sample shows a 1980 turnout rate of 60%. While this is surely something of an exaggeration, it should be noted that the 52.6% official turnout figure reported by the Census Bureau understates turnout.[2]

The question addressed here is whether registration requirements are a significant deterrent to voting. The answer is clearly affirmative. Eighty-seven percent of the registered respondents in the Michigan NES sample voted in 1980. The 1980 Voter Supplement of the Census Bureau's Current Population Survey (CPS), based on unverified responses from a more representative sample of 114,944, yields an estimate that 88.6% of those registered voted. In short, those who are registered vote. This finding leads to a critical shift in perspective. Traditionally scholars have asked why people don't vote.[3] The fact that registration is virtually equivalent to voting changes the question. The problem instead is to understand why people do not register. We believe that many U.S. citizens are not registered because they have recently moved.

The bureaucratic tasks required to vote are a greater impediment for some groups than for others. This observation applies to the impediments to registration far more than those to voting itself. In Table 1 we provide convincing evidence that registration is the critical hurdle. Only 51% of citizens with a grammar school education voted in 1980, compared with 84% of those with more than four years of college, a gap of 33 percentage points. The gap, however, narrows to 17 percentage points when the comparison shifts to registered respondents. The less educated act much more like the better educated, once they have crossed the crucial barrier of registration.

This pattern is even more pronounced for different age groups. The differences in turnout among age groups almost disappear when the comparison is with people who have registered. Only 42% of those aged 18 to 24 made it to the polls in 1980, far behind the 69% turnout rate of people 35 and older. Yet among young people who managed to register, 86% voted, compared with 90% of those aged 35 to 64 and 85% of those over 64.

Most interesting of all is the relationship of political interest to turnout among those registered. People who say they hardly ever follow politics are half as likely to vote as are those who say they pay attention most of the time; half of the former group are registered, compared with 83% of the latter. Yet once people who profess no political interest do register, they are very good bets to vote. Fully 74% of them voted in 1980, compared with 91% of the avid political fans who were registered.

Who Moves?

In order to understand how many people are affected by the obstacle of registration, it is important to emphasize that

Table 1. Turnout and Registration by Age, Education, and Political Interest, 1980

Participation Variables	Percent of All Respondents Who Voted	Percent of Respondents Registered to Vote	Percent of Registered Respondents Who Voted
Education			
0–8 years	51	65	79
9–12 years	53	64	84
1–4 years college	68	75	90
5 or more years college	84	87	96
Age			
18–24	42	49	86
25–34	57	65	88
35–64	69	77	90
65 and over	69	82	85
Political interest			
Hardly at all	38	51	74
Only now and then	54	64	84
Some of the time	70	76	92
Most of the time	75	83	91
All U.S.	60	69	87

Source: 1980 National Election Study.

very large numbers of U.S. citizens have lived in their present residences for fairly short periods of time. Between March 1980 and March 1981, 16.6% of the voting age population, nearly 27 million people, moved. Most of them (62%) moved within the same county, another 22% stayed in the same state, and the remainder moved to another state (U.S. Bureau of the Census 1983a). Mobility was even higher 20 years earlier. The one-year mobility rate was 20% in 1960/61 and 17.9% in 1970/71 (U.S. Bureau of the Census 1984). Of course, some people are more likely than others to move and, once moved, to move again; at any time the population of movers includes both those who are good bets to stay put and others who will move again and again. Even so, we were surprised to find that 32.8% of the 1980 Michigan NES sample reported that they had lived in their current residences for two years or less. It is these people—a third of the sample—whose characteristics we will examine. Staying put for more than two years seems to be something of a dividing line in the relationship between mobility and turnout (Wolfinger and Rosenstone 1980, 52).

Table 2 summarizes the more relevant demographic and attitudinal characteristics of movers and stayers (people who had lived in the same place for more than two years). The most striking thing about the movers is their youth. Sixty-eight percent of the movers, while only 23% of the stayers, were under age 35. Perhaps equally predictable is the somewhat higher educational level of the movers. Forty-four percent—compared with 33% of the stayers—had attended college. The difference in income between the two groups is almost marginal. Twenty-six percent of the movers and a third of the stayers enjoyed family incomes of at least $25,000 in 1979. The racial character of the two groups is identical. Finally, the movers are overwhelmingly renters, while

Table 2. Movers' Demographic and Attitudinal Characteristics

Demographic and Attitudinal Attributes	Percent of Those Living 2 Years or Less at Current Address	Percent of Those Living More Than 2 Years at Current Address
Turnout	48	65
Age		
18–24	30	7
25–34	38	16
35–64	27	54
Over 64	6	23
Education		
0–8 years	6	15
9–12 years	50	51
1–4 years college	33	26
5 or more years college	11	7
Family income		
$0–9,999	27	25
$10,000–14,999	18	15
$15,000–24,999	29	27
$25,000–34,999	14	18
$35,000 or more	12	15
Race		
White	86	88
Black	11	12
Other	2	1
Housing		
Rent	54	19
Own	46	81
Political activities[a]		
None	88	87
One or more	12	13
Political interest		
Hardly at all	16	15
Only now and then	27	22
Some of the time	35	35
Most of the time	22	28

[a] The four political activities used in this index were (1) attendance at political meetings, (2) work for a party or candidate, (3) display of a campaign button or sticker, (4) membership in a political organization or club.

Source: 1980 National Election Study.

most stayers own their own homes.

This thumbnail sketch offers reasons for differing expectations about movers' turnout. Their youth argues for light voting, their higher educational attainment hints at a contrary effect. That so many movers rent suggests lower voting rates since people who own their own homes turn out at a higher rate than do renters (Kingston, Thompson, and Eichar 1984, 131–50). With so many variables, multivariate analysis is called for to identify the relationship of any particular variable to turnout.

At least three published studies fill the bill; all agree that mobility strongly depresses turnout (Cassel and Hill 1981; Verba and Nie 1972, 139–46; Wolfinger and Rosenstone 1980, 50–54). The most thoroughgoing of these studies demonstrated that, with demographic variables and registration laws held constant, people who had lived at their current addresses for less than a year before the 1974 election had a turnout rate 22 percentage points lower than those who had not moved for at least 10 years. People who had stayed put for 1 to 2 years were 19% lower, and even those who had lived in their current homes for 3 to 5 years were nine points lighter voters (Wolfinger and Rosenstone 1980, 52).

There was a time when movers faced residency requirements of up to two years, which were quite an obstacle to quick resumption of the voting habit. These onerous laws were struck down by the Supreme Court in 1972 (*Dunn v. Blumstein* 1972). In almost every state the residency requirement is now 30 days or less. Movers are still light voters despite the removal of significant legal barriers because of the need to reregister and the low priority that this action has for them. People in a new home have all sorts of arranging and adjusting to do, from redecorating to mastering innumerable details of domestic administration: where to shop, how to get to work, how to spend recreational time. Registering to vote is drab and boring, a weak claimant for attention. What is more, half of all moves occur from June through September (Heiden 1981, 48–51). Such summer movers have little time to register before the deadline, which is most commonly 30 days before the election, that is, the first week in October. In short, movers do not face legal obstacles to registering, but this step takes a back seat to the many things that must be done when trying to settle down in a new home.

Although movers are the biggest light-voting group in the country, virtually no attention has been given to their characteristics, other than to note that they are mostly young. We begin our analysis with a point of great importance: while movers are much less likely than stayers to vote, they share with them most of the interests, attitudes, and forms of behavior that are usually associated with voting. Movers are just as likely as stayers to talk about the campaign, pay attention to political items in the news, think they have a say in politics, deny that politics is too complicated to understand, and assert that public officials care what people like themselves think. They are almost as likely to care which party wins the presidential election, to be interested in the campaign, and to follow politics most or some of the time. They are equally likely to engage in political activities other than voting. These findings would lead us to expect movers to vote as much as stayers, but only 48% of them voted in 1980, compared with 65% of the stayers. Even those movers who participate in politics are much lighter voters than activist stayers.

A Methodological Interlude

The data source on which we have relied up to this point is the 1980 NES conducted by the University of Michigan Center for Political Studies. The standard source of most academic research on voting behavior and public opinion for 30 years, the NES includes dozens of questions about respondents' attitudes toward candidates, issues, parties, and the political process itself; as well as many items on aspects of political interest and behavior. Moreover, we do not have to rely on respondents' claims about registration and voting; these points were checked at the relevant county records office. One disadvantage of the NES is its sample size: with 1,437 cases in the vote validation sample, few analyses are possible in which individual states are the unit of analysis. By the same token, some important demographic categories are so sparsely populated as to engender anxiety.

Table 3. Mobility by Vote and Registration in 1980

Years in Current Residence	Percent Who Voted	Percent Registered	Percent of Registered Who Voted
0–2 years	48	56	87
3–5 years	58	66	88
6–10 years	60	73	82
11 or more years	72	82	88
Total	60	69	87

Source: 1980 National Election Study.

Sample size is the least of our worries with our second data source: the Voter Supplement of the Census Bureau's CPS. The CPS includes all the relevant demographic questions found in the NES, but its political content is limited to questions on registration and turnout, and these are not validated. Its advantages are a somewhat more representative sample (see Appendix) of enormous size: 114,944 cases even after we deleted noncitizens and cases where the interviewer did not ascertain if a vote had been cast. This great number of cases enables us to analyze the behavior of respondents in states with election day registration and other unusual state-level contextual variables. We can, moreover, achieve unusually precise population estimates, even for small groups. We will present analyses using both surveys when this allows us to profit from the advantages of each. Should the different samples give us similar findings, we would have increased confidence in our conclusions.

In our analysis we have chosen to use turnout as the dependent variable. As discussed earlier registration and turnout are almost the same thing. We have performed multivariate analyses using both registration and turnout as dependent variables. Residential mobility as an independent variable has a large and statistically significant impact on both turnout and registration. Overall, there is little difference between explaining registration and explaining turnout. We report our analysis on turnout, rather than registration, because turnout is the variable of interest.

The dependent variable in our analysis —turnout—is dichotomous; therefore the most appropriate statistical technique to measure the effect of any independent variable—mobility, education, age—would be one that specifies the nonlinear form of the relationship and makes correct assumptions about error terms. Consequently we have employed probit analysis in this paper.

The Effect of Mobility on Turnout

As a preliminary step, we examine the simple bivariate relationships between mobility and registration, and mobility and turnout. In Table 3 we display these relationships. We show that 48% of people who had lived 2 years or less in their residences voted in 1980, compared with 58% of those who had stayed put for 3 to 5 years and 60% for those in the 6-to-10 year category. Turnout leveled off only among people who had lived in the same place for more than 10 years, 72% of whom voted. The data are very similar for registration, as shown in the middle column of Table 3. In the right column we show that the percentage of the registered who voted did not vary appreciably by

Table 4. Probit Estimates of the Effect of Variables on Turnout in 1980

Variable[a]	National Election Study Coefficient	Current Population Study Coefficient
Closing date	-.032 (.007)	-.010 (.0004)
Age	.030 (.016)	.048 (.001)
Age squared	-.00016 (.00017)	-.0003 (.00001)
Income	.018 (.010)	.0007 (.00004)
Rent/own	.137 (.114)	.194 (.011)
Education squared	.005 (.0008)	.006 (.00007)
Mobility	.057 (.013)	.084 (.002)
Single/married	.223 (.104)	.189 (.010)
Other/full absentee registration		.069 (.011)
Political interest	.210 (.046)	
Local elections important/not important	-.572 (.136)	
Public officials care/don't care what respondent thinks	-.226 (.092)	
Constant	-1.700	-2.494
N	1,005	103,575
Percentage correctly predicted	72.3	72.5

[a]The coding for each of the variables is given in the Appendix.

Note: We deleted from the probit equation any respondents for whom information was not available on all variables used in the equation. The numbers in parentheses are the standard errors.

mobility. Even among the movers, 87% of the registered went to the polls.

Our first major task is to examine the relationship between mobility and turnout, with other independent variables controlled. The probit coefficients for both the NES and CPS samples are presented in Table 4. We show that mobility has a substantial and statistically significant impact on turnout when all other variables are controlled. This finding is consistent with other research and thus is not particularly surprising.

More noteworthy are the strong positive influences of home ownership and marriage.[4] These effects are independent of age, income, and mobility (and attitudinal variables in the NES sample). We think that homeowners are more likely to feel a stake in the society and feel connected to their community, and that these ties account for their higher turnout. We distinguish between this sense of being rooted and the more plausible explanation of the stayers' higher turnout: more time to take care of the administrative chore of registering. Most movers, after all, were already living in the same county before they shifted residences, and thus are not likely to need an extended period of time to become familiar with local issues and candidates. Moreover, we are talking about participation in a presidential election, where knowledge of the relevant issues and candidates and identification with the appropriate symbols is not lost by moving from one town or state to another.

By the same token, married people face the same registration obstacles as the unmarried, yet they are more likely to vote. They have an inherent support and motivational system that single people lack (Campbell, Converse, Miller, and Stokes 1960, 109; Wolfinger and Rosenstone 1980, 44–46). Information costs may also come into play. Only one spouse need invest time and energy in learning how and where to register after a move, but both partners benefit. A single person does not enjoy this advantage.

The equation using NES data is more fully specified because of the attitudinal variables not available in the census data. Political interest, "external" political efficacy ("I don't think public officials care much what people like me think"), and belief in the importance of voting in local elections are all substantively and statistically significant in their relation to turnout. (Although a belief that voting in most local elections is unimportant decreases the probability of voting by a whopping 18%, it should be noted that only 12% of the sample holds this view.)

Variations in the Effect of Mobility

Having established that mobility depresses turnout, we next investigate variations in that effect. Is everyone's probability of voting equally depressed by moving, or do some kinds of people respond differently to the experience? There are at least three dimensions on which movers vary and that seem likely to affect their probability of voting. First, they differ in their demographic and attitudinal attributes. Second, their life situations are not the same: some movers are single, some married; some are homeowners, some are renters. Finally, the legal environment into which they move will vary from state to state with regard to registration requirements.

We begin by examining the relationship of mobility to turnout at different levels of education. In Tables 5a and 5b we show that the impact of moving varies inversely with education. College graduates are decidedly less hindered by moving than people with less education. With all other variables in the NES equation held constant (see Table 5a), people with college degrees who had moved within six months of the interview were 13 percentage points less likely to vote than if they had lived in the same place for at least 11 years. Those with less than a high school education in the same situation were 21 percentage points less likely to vote.[5] In other words, moving has a larger impact upon those with a lower probability of voting in the first place. Among people with six months or less residential stability, those without a high school diploma have a 37% probability of voting, compared with a 77% probability for college graduates.

In Table 5b we show much the same pattern. Using the CPS sample, we have enough cases to break down the categories more finely. People with some graduate education are less affected by mobility than are those with just a college degree. Indeed, they so quickly begin voting that there is scant room for improvement as the months in their new homes lengthen into years. Their turnout rate after six months is only seven percentage points below their rate at maximum residential stability. In both tables the probability of the best-educated people voting increases modestly with greater residential stability. Well-educated movers seem to hit the ground running; even just after changing their address they still turn out at 88% in the CPS study.

Table 5a. Effect of Mobility on Turnout, by Education, in 1980 (National Election Study Data)

Years of Education	Differences between Probability of Voting after 11 or More Years Stay and after the Following: Less than Six Months	6–36 Months	4–6 Years	7–9 Years	10 Years	Probability of Voting after 11 or More Years Stay[a]
11 or less (221)	-21	-18	-12	-6	-2	58
12 (387)	-21	-17	-11	-5	-2	71
13-15 (208)	-19	-15	-10	-5	-2	78
16 or more (189)	-13	-10	- 6	-3	-1	90
Total (1,005)	-19	-15	-10	-5	-2	73

Two other aspects of the effect of education emerge in Table 5. First, while each increment in education tends to raise the likelihood of voting, the sharpest break is between those who have obtained a college degree and those who have not. People who did not attend high school, high school graduates, and college dropouts increase their probability of voting by almost identical amounts as they remain longer in their residences. Turnout of college graduates increases more slowly. Second, the familiar relationship between education and turnout is strongest for recently moved citizens and progressively declines among those who have stayed put for longer periods. That relationship never comes close to vanishing, but it is weaker among long-term residents than among the more mobile respondents.

The relationship of mobility to turnout by political interest is almost identical to that by education: the lower the interest, the less turnout among recent movers and the more residential instability reduces the probability of voting. The relationship of interest to turnout declines progressively as stability increases, although it remains strong even among the most stable.

Table 5b. Effect of Mobility on Turnout, by Education, in 1980 (Current Population Survey Data)

Years of Education	Difference between Probability of Voting after 6 or More Years Stay and after the Following: Less than One Month	1–6 Months	7–11 Months	1–2 Years	3–5 Years	Probability of Voting after 6 or More Years Stay[a]
0–8 (12,624)	-19	-18	-17	-6	59	
9–11 (13,815)	-18	-17	-16	-14	-6	64
12 (39,125)	-17	-16	-15	-13	-6	71
13–15 (20,370)	-16	-15	-14	-12	-5	77
16 (10,024)	-11	-11	-10	- 8	-3	89
17 or more (7,617)	- 7	- 7	- 6	- 5	-2	95
Total (103,575)	-16	-15	-14	-12	-5	73

Note for 5a and 5b: The entry in each cell is the probit estimate of the effect on turnout of living at one's current residence for the indicated length of time compared to the turnout of those in the same educational group at 11 years of residential stability for the NES sample or 6 years for the CPS sample.

[a] The probability of voting of an individual with the indicated amount of education who had lived in the same place for 11 (or 6 years, with all the other variables in the equation controlled.

While mobility accentuates the impact of education and political interest on turnout, this is not the case with age. Moving has a strong and fairly similar negative effect on the turnout of *all* age groups. A very recent change of address decreases the likelihood of voting by 18 percentage points for people aged 18 to 21 and has only slightly less effect on older people. Across all age groups the probability of voting increases with stability to about the same extent.[6] In summary, mobility has a strong and differentiating effect on people with different levels of education and political interest and, somewhat surprisingly, a strong but uniform effect on those at different ages.

Movers differ not only in their demographic and attitudinal attributes but also in their life-styles. As we saw, with all other demographic variables controlled, renters are considerably less likely to vote than are homeowners. Is this effect accentuated by how recently these individuals have moved? Apparently not. Renters and homeowners are both strongly affected by moving but to about the same extent. This is not to say that their probabilities of voting are the same. At 11 years of stability, renters have a 64% probability of voting compared with 77% for owners. Both mobility and renting have a substantial and independent depressive effect on turnout. Even the most recently moved homeowners vote as much as renters who have stayed in the same dwelling for 7 to 9 years. The relationship between ownership status and turnout seems to have different causes than that between mobility and turnout. The latter reflects the cost side of a cost-benefit analysis of turnout: the administrative hurdle of registering impedes immediate resumption of the voting habit. But once we have controlled not only for mobility but for all the other variables in our probit equation (including education, interest, and income), it is difficult to see how renters face any impediments to registering and voting not experienced in equal measure by homeowners. The explanation would seem to lie on the benefit side: renters are less impelled to register, perhaps because of weaker feelings of societal membership.

The relationship between marital status and mobility and turnout is virtually the same as that with homeownership. Married people are heavier voters; the impact of moving on their likelihood of turning out is faintly weaker than for single individuals.[7] In short, the relationship of these life situation factors and turnout is compounded when mobility is added to the picture. Since this is true with everything else controlled, it suggests that being either a homeowner or married gives one a larger personal stake in the community, or more motivation to register and vote.

Finally, the legal environment in which movers find themselves differs from state to state. We argue that movers are light voters because of the need to reregister and the low priority of doing so. This leads us to expect the impact of mobility to decline as the importance of registration declines. This diminution would be most marked in the four states that permit election day registration—Maine, Minnesota, Oregon, and Wisconsin—and in North Dakota, which does not require registration at all. Turnout in these five states is far above the national level. In 1980, when 52.6% of the U.S. voting age population went to the polls, the comparable figures in the easy registration states ranged from 61.5% in Oregon to 67.2% in Wisconsin and 70.1% in Minnesota (U.S. Bureau of the Census 1983b, 492).

Initial analyses confirmed our hunch about the weaker depressive effect of mobility in the 5 states. A simple cross-tabulation contrasting turnout at different stages of mobility in the two sets of states showed that people who had lived in their homes for no more than six months had a 61% turnout rate in the 5 states, com-

Table 6a. Effect of Mobility on Turnout, by Ease of Registration, 1980 (National Election Study Data)

	Difference between Probability of Voting after 11 or More Years Stay and after the Following:					
States	Less than Six Months	6–36 Months	4–6 Years	7–9 Years	10 Years	Probability of Voting after 11 or More Years Stay
Easy registration (47)	-10	- 8	- 5	-2	-1	93
Other (958)	-20	-16	-10	-5	-2	72
Total (1,005)	-19	-15	-10	-5	-2	73

pared with 43% in the other 45 states. The gain in turnout with greater stability was 20 percentage points in the 5 states and 31 points in the other 45. The same point was made by analysis of ordinary least squares equations of demographic variables and turnout for the two sets of states. The relationship of mobility to turnout is almost twice as strong in the 45 states as in the easy registration five, in which more permissive laws ameliorate much of the blighting effect of moving. By the same token, the effect of other demographic variables is weaker in the easy registration states because, with the registration barrier so much lower, the cost of turnout is drastically reduced.

The strongest test of the proposition would be made with probit analysis, and here the results are not so clear. The NES sample provided a gratifying confirmation, as we show in Table 6a. With only 47 respondents in the easy registration category, however, this data set is not ideal for state-level analysis. This is the strong point of the CPS, which has 8,341 respondents in the five easy registration states. Here we were disappointed. As we show in Table 6b, the findings are in the predicted direction, but the differences between the two groups of states are much smaller, at most two percentage points. This analysis suggests that mobility deters voting almost as much in states where registration is not so great a barrier.

We have seen strong evidence that mobility has a significant impact on registration and turnout. Among the most recent movers, differences in education and political interest significantly change the probabilities of registering and voting. These relationships weaken as residential stability increases. The longer people live

Table 6b. Effect of Mobility on Turnout, by Ease of Registration, 1980 (Current Population Survey Data)

	Difference between Percentage Who Vote after 6 or More Years Stay and Percentage Who Vote after					
States	Less Than One Month	1–6 Months	7–11 Months	1–2 Years	3–5 Years	Probability of Voting after 6 or More Years Stay
Easy registration (8,341)	-14	-14	-13	-11	-4	81
Other (95,234)	-16	-15	-14	-12	-5	73
Total (103,575)	-16	-15	-14	-12	-5	73

Note for 6a and 6b: The entry in each cell is the probit estimate of the effect on turnout of living at one's current residence for the indicated length of time compared with the turnout of those in the same state at 11 years of residential stability for the NES sample or 6 years for the CPS sample.

at the same address, the more likely they are to vote, regardless of any other characteristic. Even less educated people with little interest in politics finally get around to registering if they live in the same place for several years. Differences in life-styles with regard to marital status and renting or owning one's residence also reveal a strong depressive effect on turnout as mobility increases, although the effect is fairly uniform. An early closing date for registration also depresses turnout among movers, although not as strongly as we expected.

How to Increase Turnout

If almost everyone who is registered votes, holding elections on Sunday, making election day a holiday, or extending the hours that the polls are open would do little to improve turnout. These measures are designed to facilitate people who are registered, who are already very likely to vote. Money, energy, and political capital should be invested in efforts that have more potential: getting people to register on the amply documented assumption that once people register, they vote.

We have established that movers are less likely to vote for no reason other than the need to reregister and the low priority that this action has. There is no question that election day registration would be particularly beneficial to such people. It is equally certain that this is a dead issue. President Carter's proposal for a national scheme did not even get out of a congressional committee in 1977, and there are no signs that the idea will soon be revived in Congress. In 1978 the voters of Ohio decisively rejected an election day registration plan enacted by the state legislature. This seems to be an idea whose time has come and gone.

A more feasible approach would be to concentrate on maintaining the registration of people who move. We propose a measure that would reduce the negative effect of moving on turnout by linking the maintenance of registration to an action that is usually an intrinsic part of moving. Almost all movers file a change-of-address notice at a local post office before leaving their old home. According to the postal service, 40 million such forms were filed in 1983 and again in 1984. Some of these reported temporary moves, others were used by people who moved more often than once a year. If this form were modified to permit the mover to indicate that the move included a change of voting address and carbonized so a copy could be produced simply by filling out the form, the second copy could be sent by the post office to the chief election official in the state where the move originated. In 83% of all moves, this would also be the destination state. In the case of intrastate movers who were already registered, this copy could be accepted as adequate legal authority to reregister the individual at the new address. If the mover were not registered already, receipt of the notice might stimulate dispatch of a registration-by-mail postcard.

In any case, receipt of the notice would lead to cancellation of the old address registration. This would be an efficient and inexpensive way of removing deadwood from the registration rolls, thus eliminating perhaps the biggest opportunity for election fraud. Pruning the rolls would reduce the costs of election administration and of direct mail campaigning. Accurate registration records would also make jury selection more efficient and have a beneficial effect in those states where a proportion of the number of registered voters is used as a threshold for successful bond issue referendums or for signatures to qualify a proposition for the ballot.

We began to advocate this idea in September 1983, concentrating on attracting the attention of officials at the federal level. Action by the federal government is a necessary but not sufficient condition for enacting this proposal, which could be implemented only in a state that made the necessary changes in its voter registration laws. People who heard the proposal liked it, but at first there was a very small audience. In March 1985 a bill (HR 1668) to accomplish the federal aspect of our proposal was introduced by Representative Mel Levine (D.-CA), with three Republican and four Democratic initial cosponsors. A similar bill, S 1439, was introduced later that year by Senators Christopher Dodd (D.-CT) and John Chafee (R.-RI).

The first legislative action was in the House. Over two dozen more Representatives had cosponsored HR 1668 by July 1985, when it was the subject of a hearing by the Subcommittee on Postal Operations and Services of the Committee on Post Office and Civil Service. The chairman, Mickey Leland of Texas, and the ranking minority member, Frank Horton of New York, were both strongly favorable, as were two testifying secretaries of state, March Fong Eu, a Democrat from California, and Susan L. Farmer, a Republican from Rhode Island. Leaders of three groups interested in expanding voter turnout supported the bill but expressed fear that some states might use the procedure to purge names of movers and pay little attention to reregistering them. The postal service witness, the only outspoken critic, was subjected to skeptical questioning by Leland and Horton.

After the hearing there were several rounds of negotiations with the three voter registration groups on the one hand and with several state and local election officials on the other. The former wanted to avoid purging without reregistration and were untroubled by deadwood and administrative feasibility, major concerns of even the most progressive election officials.

Several criteria are important as policy makers ponder this proposal: cost, administrative feasibility, and political implications. The postal service, which is not motivated to minimize the difficulties, has produced several estimates of the cost of sorting change-of-address notices and transmitting them to the appropriate election official. In the fall of 1985, the most recent and lowest estimate was 30¢ per form. If every state participated in the program, the total cost of postal service processing would be $12 million. No state could participate without enacting its own implementing legislation, however. This process would take years and some states doubtless would never choose to join in. Therefore the $12 million figure is an upper limit that will probably never be attained. Also, this cost will be mitigated by substantial savings in printing and postage as deadwood is eliminated. Partly because the savings would be realized by the states and partly because of the mood of fiscal stringency in Washington in the mid-1980s, HR 1668 provided that these administrative costs would be borne by participating states.

The plan's administrative feasibility is attested by the endorsement of HR 1668 by 11 secretaries of state, a number of individual county clerks, the National Association of County Recorders and Clerks, and the legislation committee of the California County Clerks Association. On the other side of the ledger is a resolution of opposition passed by the Florida State Association of Supervisors of Elections.

HR 1668 was reported in June 1986 by the Subcommittee on Postal Operations and Services. In September the full Committee on Post Office and Civil Service, by a unanimous voice vote, ordered the bill reported. For a moment it appeared that HR 1668 might pass the House on suspension of the rules and—just possibly—be attached to a bill on the Senate floor. Instead, it became a casualty of the election-year rush to adjourn. At the time of writing, its prospects in the 100th Congress seem bright.

Political Impact

And then there is the political question, How would the interests of the two parties be affected by easing the effects of moving on turnout? This question can be answered in large measure by further analysis of our data to see how a voting population enlarged by weakening the relationship of mobility to turnout would differ from the current voting population. Before considering the political implications of our proposal, we will see how effective this proposal would be in increasing turnout.

We begin by estimating the turnout rate that would result from eliminating the effect of mobility on turnout. How many people would vote if everyone participated at the same rate as the most stable segment of the population? This question cannot be answered simply by assuming that the mobile would vote as much as the stable because the two groups differ with respect to other variables affecting turnout. Among other things, movers are predominantly young renters, two characteristics associated with low turnout. We can use the equations in Table 4 to make the appropriate forecasts. After 10 years in the same residence, turnout no longer increases with additional stability. Therefore we estimated what the turnout rate of the sample would be if everyone voted as if they had lived at the same address for 11 years, as opposed to their current probability of voting. On the basis of the NES probit equation, we estimate that the national turnout rate would increase by nine percentage points under these conditions.[8]

This figure defines the outer limit of our proposal's potential consequences; indeed, it doubtless exaggerates them. Enactment and nationwide implementation of HR 1668 would not eliminate the relationship between residential mobility and turnout, if only because of interstate moves, which would be beyond the reach of our proposal. Even a nine-percentage point gain in turnout would not alter this country's next-to-last position in international rankings (Wolfinger, Glass, and Squire 1985) because mobility is by no means the only explanation for low U.S. voting rates. Nevertheless, an increase of nine, or seven, or five percentage points would be a major accomplishment, comparable to the gains produced by election day registration and without any risk of election fraud.

Other than increasing the number of voters, what consequences would ensue from eliminating the effect of mobility on turnout?[9] The proper comparison to answer this question is between actual voters and the hypothetical larger voting population.[10] The characteristics of the actual and projected voting populations are summarized in Table 7. In most instances, the two groups are almost identical. The projected voters would be a bit younger, with people under the age of 34 accounting for 32%, compared with their 28% share of those who actually voted in 1980. This is the only difference in demographic characteristics that exceeds sampling error. Other differences are both statistically and substantively insignificant. People without college degrees would account for 77% of all voters, in contrast to the actual 75%. People with family incomes under ten thousand dollars would increase from 18% to 19.2%.

Some people concerned with voting—including some officials who administer elections—deplore efforts to "make voting too easy" because they believe that a certain amount of difficulty screens out those citizens whose commitment to electoral participation is unacceptably weak. Whatever the merits of this argument as prescriptive political theory, its relevance to our present concerns is limited because HR 1668 would affect only people who had registered before they moved. This is not, however, tantamount to saying that if the mobile voted at the same rate as the stable, the voting population would dis-

Table 7. Comparison of Actual Voters to Projected Voters at Eleven Years of Residential Stability, 1980

Characteristics	Percent of Actual Voters	Percent of Projected Voters
Age		
18–21	4	4
22–27	11	14
28–33	13	14
34–70	63	60
71 and older	8	8
Education		
11 years or less	17	17
12 years	37	38
13–15 years	21	22
16 or more years	25	23
Income		
$0–$9,999	18	19
$10,000–$19,999	28	28
$20,000–$29,999	28	28
$30,000–$49,999	19	18
$50,000 or more	7	7
Party Identification		
Democrat	49	49
Independent	10	12
Republican	41	39
Other/apolitical	0	1
Self Identified Ideology		
Liberal	20	18
Middle-of-the-road	19	20
Conservative	34	34
Haven't thought much/don't know	27	28
Political Interest		
Hardly at all	10	10
Only now and then	20	22
Some of the time	38	37
Most of the time	33	32

Source: 1980 National Election Study.

play an equal level of civic virtue because those movers who most quickly return to voting are likely to be the most alert and concerned part of the mobile population. They have, after all, surmounted a barrier to participation not faced by the stayers. If the costs of moving were reduced, they might be joined at polling places by movers with somewhat less civic virtue, but still, perhaps, no less virtue than stayers.

The answer to these speculations is in Table 7, in which we show that the least politically concerned citizens, those who say they follow politics "hardly at all," would account for the same 10% of the projected expanded voting population that they do of actual voters. People with intermittent interest in politics would be 22% of the expanded population, up two percentage points from their share of actual voters; and the proportion of people who follow politics some or all of the time would drop two points. None of these differences approaches sampling error, and we conclude that HR 1668 would not lower the political interest of the U.S. voter.

The most interesting question, of course, is the political consequences of expanding the electorate by reducing the effect of mobility on turnout. We can calculate the number of Democrats and Republicans in the projected larger voting population and compare that number to the partisan distribution among actual 1980 voters. This reveals a microscopic loss for the Democrats, from 49% of the actual voters to 48.6% of the projected voters. More noteworthy—although still below the level of statistical significance—is a loss of nearly two points for the Republicans, achieved almost entirely through a gain in the proportion of voters who are Independents. If we assume that this group splits its vote evenly, the net damage to the Republican party is more on the order of one percentage point.[11] It is true that elections have been won by a smaller margin and that no politician likes to give up any advantage, even a small one. Yet no responsible analyst of survey data would take seriously so tiny a gap between respondents in a sample reduced to barely a thousand cases.

Our conclusion about the evanescent character of the apparent loss in Republican identifiers is buttressed by the findings for self-identified liberals and conservatives, which are also in Table 7. In contrast to a two-percentage point loss for Republicans, it is liberals who suffer a similar decline from the actual to projected voters. The proportion of conservatives remains the same. The liberal loss comes from middle-of-the-road respondents and those who say that they have not thought enough about liberalism and conservatism to make a choice. None of these differences in partisan and ideological identification comes close to statistical significance. We cannot find much in these findings to alarm either party.

Conclusion

Movers resemble stayers on the motivational variables that are directly related to voting: interest in politics, attention to the campaign, concern about the outcome, and political efficacy. These similarities support our conclusion that movers' lower turnout reflects the administrative burden of registration rather than lower civic virtue. We would expect movers to vote more if the burden of reregistration were eased. We have proposed a method to accomplish this and estimate that if the effect of mobility on turnout were removed, turnout would increase by nine percentage points. Expanding the voting population in this way would produce no consequential advantage for either party.

Appendix

1980 Vote Validation Study

The Vote Validation Study covered respondents in the conventional American National Election Study (NES) time series sample (N = 1,614). This sample was to be interviewed before and after the 1980 election; those whose postelection interviews were not completed were, nevertheless, included in the Vote Validation Study. For a variety of reasons, information on registration status or voting could not be gathered on 177 of the 1,614 respondents. We have deleted these cases from our analysis (categories 0, 4, 5, 6, 7, 8, 9 on variable 1207), which is limited to the 1,437 respondents for whom validated data are available on both registration and voting. See the relevant codebook: University of Michigan Center for Political Studies (1982, 712–13).

Coding of Variables Used in Study

1980 National Election Study. Closing date: 0 = living in state with election day

Table A-1. Samples for the Study of Registration and Turnout (in percentages)

Characteristics	1980 Census	1980 Current Population Survey	1980 National Election Study Vote Validation Study
Age			
18–24	18.4	17.8	14.2
25–34	22.9	22.7	23.0
35–64	42.9	44.1	46.0
65 and over	15.7	15.5	16.8
Education			
0–12 years	68.2	63.6	61.9
1–4 years college	25.3	29.1	28.6
5 or more years college	6.5	7.3	9.4
Income			
$0–9,999	27.5	27.4	24.7
$10,000–14,999	14.9	17.9	15.3
$15,000–24,999	25.5	28.8	28.0
$25,000 or more	32.1	25.9	32.0
Race			
White	83.4	88.2	88.2
Black	11.7	10.6	10.6
Other	4.9	1.2	1.2

Sources: The 1980 census figures for age and income are calculated from the U.S. Bureau of the Census 1983c. The numbers for education and race are calculated from Census of Population and Housing, 1980: Summary Tape File 3C (Machine-readable data file), prepared by the Bureau of the Census 1981. All of these figures are for the resident population *including noncitizens.* The figures for the CPS and the 1980 NES are for citizens 18 years old or older.

registration; coded value for other states was number of days from close of registration to election day (Council of State Governments 1980).

Age (variable 1179): respondent's age in single years, 18 to 97.

Income (variable 686): coded in 22 categories as done by NES with 1 = less than $2,000 and 22 = $50,000 and over.

Homeownership status (variable 719): 0 = rent or other financial arrangement; 1 = own.

Education squared: the square of the value coded for education (variable 429), which ranged in single years from 0 to 17.

Mobility (variable 718): less than 6 months = .25; 6 to 12 months = .75; 1 to 2 years = 1.5; 2 to 3 years = 2.5; 3 to 4 years = 3.5; 4 to 5 years = 4.5; 5 to 6 years = 5.5; 6 to 7 years = 6.5; 7 to 8 years = 7.5; 8 to 9 years = 8.5; 9 to 10 years = 9.5; more than 10 years = 11.

Marital status (variable 409): 0 = never married, divorced, separated, widowed, common law, other; 1 = married.

Political interest (variable 974): 1 = hardly at all; 2 = only now and then; 3 = some of the time; 4 = most of the time.

Importance of local elections (variable 146): 0 = disagree (respondent does think local elections are important); 1 = agree.

Efficacy (variable 1033): 0 = disagree (efficacious); 1 = agree (not efficacious).

1980 Current Population Survey. Marital status: 0 = not married; 1 = married.

Absentee registration: 0 = living in state with no absentee registration provisions; 1 = living in state with absentee registration provision.

Closing date: same as NES.

Age: respondent's age coded in single years.

Income: under $1,000 = 5; $1,000 to $1,999 = 15; $2,000 to $2,999 = 25; $3,000 to $3,999 = 35; $4,000 to $4,999 = 45; $5,000 to $5,999 = 55; $6,000 to $7,499 = 67; $7,500 to $9,999 = 88; $10,000 to $11,999 = 110; $12,000 to $14,999 = 135; $15,000 to $19,999 = 175; $20,000 to $24,999 = 225; $25,000 to $49,999 = 375; $50,000 and more = 600.

Homeownership status: 0 = do not own home; 1 = own home.

Mobility: less than 1 month = .041; 1 to 6 months = .292; 6 to 11 months = .75; 1 to 2 years = 1.5; 3 to 5 years = 4; 6 years = 6.

Education squared: The square of the respondent's coded value for education, ranging from 0 to 18 years.

Notes

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1. This figure is from the Vote Validation Project of the 1980 National Election Study (NES), which is described in the Appendix. For comparisons of U.S. and foreign performance on different measures of turnout, see Glass, Squire, and Wolfinger (1984) and Wolfinger, Glass, and Squire (1985). For a multivariate analysis demonstrating that the greatest part of the disparity in turnout between the United States and other countries reflects U.S. registration procedures, see Powell (1986).

2. Part of this underestimation is a result of the failure to count some ballots we have described. A more important source of underestimation is the denominator of the official turnout percentage: the entire population age 18 and above, *including noncitizens.* Deleting noncitizens from the denominator of the percentage calculation for the 1972 election raised the estimated turnout by 1.2 percentage points (Wolfinger and Rosenstone 1980, 116). The noncitizen proportion of the population has grown considerably since then. This trend alone may be responsible for the decline in official turnout from 55.2% in 1972 to 52.6% in 1980. We cannot duplicate the 1972 correction for 1980 because we have not succeeded in obtaining from the Census Bureau an estimate of the voting age citizen population in 1980. Deleting the noncitizens from the Current Population Survey (CPS) increases the estimated 1980 turnout by three percentage points (U.S. Bureau of the Census 1982, 5).

3. For an exception, see Erikson (1981). He, however, came to a very different conclusion from ours:

> Thus, one reason that people vote may be to stay registered. . . . Remove the cost of registration, and some who otherwise would have voted stay home because it is no longer necessary to vote in order to stay registered. (p. 274)

4. Despite their marginal levels of statistical significance, variables for homeownership, age, and age squared were kept in the NES equation for two reasons. First, we had strong theoretical grounds for including them in our equations and the sign of each of the suspect coefficients is in the expected direction. Second, the CPS coefficients for the variables are statistically very significant, which strengthens our belief that they belong in our analysis.

5. Wolfinger and Rosenstone (1980, 54) found that moving within a year of the 1974 election had a slightly greater negative effect on the turnout of the college-educated, a difference that disappeared after two years of stability. We have no explanation of this disparity, other than noting that the newer finding is more compatible with the theoretical approach in both studies and that the 1974 finding is based on a subsample of 7,936 cases, while our probit analysis of the CPS sample employs 103,575 cases.

6. Wolfinger and Rosenstone (1980, 52–54) found that the effect of mobility on turnout increased with age.

7. We also examined the relationship between both homeownership and turnout, and marriage and turnout. Unmarried renters have the lowest probability of voting and are hardest hit by moving, while married homeowners have the highest turnout

rate and are a bit less affected by changing their residence. Single owners and married renters are in between, turning out 9 percentage points above single renters and 11 points less than married owners.

8. The most stable mobility category in the CPS data is six years or more, hence for this data set we were only able to estimate the increase in turnout if everyone voted as if they had lived in the same place for six years, as opposed to their actual probability of voting. Under this condition the national turnout rate would rise by 5.5 percentage points.

9. What groups of respondents are to be compared to each other to answer this question? Because the status quo is an obvious reference point, one group is actual voters. If we were interested in the characteristics of those people deterred by mobility from voting, the other comparison group would consist of the hypothetical 9%. But the question we are trying to answer is different: How would elections change if mobility no longer depressed turnout? We answer this question by comparing actual voters not to the potential 9% increment but to the hypothetical larger voting population created by adding the 9% to existing voters. Because a measure like HR 1668 would not eliminate the depressive effect of mobility, this method produces somewhat inflated effects, but we have no alternative. We calculated the characteristics of projected voters by the method used by Wolfinger and Rosenstone (1980, 139).

10. One factor is beyond quantitative calculation. One or the other party may be able through its own efforts to reregister supporters who move. If this were the case, enactment of our proposal would, by governmental action, do for both parties something that one party could do for itself. This is not a wholly hypothetical observation. In some places Republicans are using their superior resources and organizational coherence to identify movers likely to be Republican and also movers likely to be Democrats, whose continuation on the registration rolls can then be protested. If these practices become an important part of Republican calculations, our proposal's bipartisan appeal will be limited to its effectiveness as a scourge of deadwood. This might not be negligible, particularly in those places where Republicans believe that large-scale exploitation of deadwood for fraudulent purposes is something of a Democratic monopoly.

11. In point of fact, Independents usually vote predominantly for the winning presidential candidate (Keith, Magleby, Nelson, Orr, Westlye, and Wolfinger 1986).

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VOTING OUTCOMES IN THE 1984 DEMOCRATIC PARTY PRIMARIES AND CAUCUSES

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Scholarly inquiry concerning influences on electoral outcomes in the presidential nomination process, though extensive, has been conducted almost exclusively with data collected at the individual level of analysis. The Michigan model of normal vote analysis suggests that long-term influences measured at the aggregate level, such as the sociodemographic, economic, and ideological characteristics of the states, are also important in determining electoral outcomes. We present an aggregate-level analysis of state characteristics that affected the Hart, Jackson, and Mondale vote proportions in the 1984 Democratic caucuses and primaries. Our primary election models explain between 65% and 83% of the variance in candidate vote shares, with sociodemographic and economic factors as the leading indicators. In the caucuses, we find that campaign spending and sociodemographic influences are dominant in models that explain between 38% and 81% of the variance. We conclude with a brief discussion of what our findings mean for future Democratic candidates.

Presidential elections have always been the centerpiece of American democracy. However, it is only recently that the public has been allowed both to determine the party nominees and to select between them in the general election. Presidential nominations, once brokered among a few party leaders, are now largely determined by the millions of party followers who participate in state primaries and caucuses. As the presidential nomination process has become more popular it has also become more complex. Several related research programs have attracted and organized scholarly efforts to understand the dynamic processes underlying selection of presidential nominees within the major parties. One of the principal issues dividing analysts has been whether to study influences on electoral outcomes at the aggregate or the individual level of analysis.

Aggregate-level studies focusing on turnout and representation have become common. Various factors explaining differences in voter turnout in primaries have been identified (Norrander and Smith 1985; Ranney 1972, 1977; and Schier 1982), while studies of representation have examined whether presidential primary and caucus electorates are representative either of the total electorate or of party identifiers (Crotty and Jackson 1985; Lengle 1981; Marshall 1978, 1981; Ranney 1972, 1977). In addition, other aggregate-level studies have examined the possible impact of campaign expenditures upon primary and caucus outcomes

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(Goldstein 1978; Orren 1985). Though aggregate-level studies have dealt extensively with aspects of the electoral process such as rates of participation among sociodemographic groups, turnout levels, and campaign spending levels, analysts have not taken the further step of asking what impact aggregate-level influences actually have on candidate vote shares.

A second line of scholarly inquiry, operating almost exclusively at the individual level of analysis, has concentrated on vote choice. Studies of individual vote choice have attempted to assess the relative importance of candidate strategies (Aldrich 1980a, 1980b; Brams 1978; West 1984), voter predispositions, candidate images, issues, and ideology (Bartels 1985a, 1985b; Gopoian 1982; Wattier 1983) in explaining voter decision making in presidential nomination campaigns.

While analyses of individual-level data concerning primary and caucus vote choice enhance our understanding, they provide incomplete explanations for outcomes in the presidential nominating process because they focus only on short-term influences such as voter predispositions, candidate images, and issues. The Michigan model of normal vote analysis suggests that long-term factors measured at the aggregate level, such as the sociodemographic and economic characteristics of the states, are also important in determining electoral outcomes. These are factors that the turnout, representation, and campaign expenditure studies have clearly indicated are relevant for explaining voting outcomes in presidential primaries and caucuses.

Like Kramer (1983, 94) we are persuaded that in some cases "an aggregate-level analysis is more likely to yield valid inferences about individual behavior than one based on individual-level data." Given that a cross-sectional study of primaries and caucuses in 51 settings (the 50 states and the District of Columbia) would be impossible to perform at the individual level, this paper employs aggregate-level data to explain voting outcomes in the 28 caucuses and 27 primaries that made up the delegate selection process in the Democratic party in 1984. This approach will allow us to develop and test a model of candidate vote shares in primaries and caucuses that takes into account both long- and short-term impacts on aggregate voting outcomes.

We selected the 1984 Democratic party nomination process as the arena for our examination for several reasons: (1) most of the previous literature on the impact of reforms on the nomination process and on voter decision making has focused on the Democratic as opposed to the Republican party process, (2) the contest for the Democratic nomination in 1984 was in doubt down through the final events of early June, (3) there was no contest for the 1984 Republican party nomination, and hence Republicans were free to cross over and participate in the Democratic party process if the state rules permitted, (4) the Democratic party race was, for the most part, a three-person contest from beginning to end, thus providing a high degree of candidate stability across the various state races, and (5) the 1984 Democratic party nomination process provides a unique opportunity for assessing the changing coalitional makeup of the party. The candidacies of Walter Mondale, Gary Hart, and Jesse Jackson involved a contest over the "soul" of the Democratic party and the direction that it should take into the future.

Six Dimensions of Influence on Electoral Outcomes

In this study we will focus on six aggregate-level contextual and structural dimensions of influence on electoral outcomes in the 1984 Democratic party primaries and caucuses. Behind each dimension—the sociodemographic characteristics of the state, the political context of the state, the economic conditions existing in the state prior to the election, the electoral rules governing the conduct of the event, the in-state campaign expenditures of the contestants in the event, and the level of voter participation—lies an extensive literature. We will present findings regarding the impact on candidate vote shares of each of these six dimensions both separately and in a general multivariate model, evaluate the extent to which our findings support or conflict with earlier studies, and discuss the implications of our findings for the conduct of presidential politics within the Democratic party.

The Influence of Sociodemographic Factors

Group or sociodemographic influences are thought to be central to the political decisions that individuals make. The dominant intellectual traditions in the area of electoral behavior—the sociological and the social-psychological approaches—both have addressed the impact of group influences on electoral outcomes. The sociological tradition has focused almost exclusively on the role of group identifications in structuring electoral outcomes. Although those representing the social-psychological perspective (identified with Campbell, Converse, Miller, and Stokes 1964, 161) have been less convinced of the direct influence of group identifications on vote choice, even they observed that "members of these groups appear to think and behave politically in distinctive ways." More recently, however, Beck (1986, 259) and others have noted a "revival in the use of social group variables in voting analysis [that] is highly promising." This study will measure the impact of several major sociodemographic variables on the vote shares enjoyed by the three major candidates in the Democratic primaries and caucuses of 1984.

The Influence of Political Context

A second broad dimension from which explanations of electoral outcomes are commonly sought concerns general ideological context or political culture. Flanigan and Zingale (1983, 72) highlight the potential importance of state political culture or context as an independent influence on the presidential nominating process when they argue that "sometimes the culture and traditions of an area will reverse or reinforce the political tendencies of other social groupings to which the individual belongs; this may result in a pattern of political behavior different from that of people with similar characteristics in other parts of the country" (see also Klingman and Lammers 1984). Substantial research indicates that political culture in the states exercises a pervasive influence over both elite and mass behavior as well as over political structures and policy outputs (Kincaid 1982, 59). Therefore, we explore both mass and elite ideological orientations as well as key organizational and structural elements of the electoral contexts that Mondale, Hart, and Jackson confronted as they did battle in the primaries and caucuses.

The Influence of Economic Conditions

A general increase in interest in the impact of economic influences on politics has led students of political economy to ask precisely which aspects of the economic environment affect electoral behavior and to what degree. Much of the recent debate over the influence of economic conditions on electoral outcomes was sparked by Kramer's (1971) analysis of economic influences on congressional elections. Kramer's finding that economic fluctuations had a relatively strong effect on voting outcomes was vigorously challenged on both theoretical and operational grounds by Stigler (1973). The latter's basic point was that sensible voters should not, and did not, respond to short-term fluctuations in economic performance in making their electoral decisions. Stigler found (1973, 166) "no . . . statistically significant relationship between average income performance and share of the vote received."

The debate over the impact of economic conditions on electoral outcomes has remained largely within the parameters set for it by the initial confrontation between Kramer and Stigler (see Arcelus and Meltzer 1975; Bloom and Price 1975; Tufte 1975). After surveying this and related debates over the nature and magnitude of the impact, if any, of economic conditions on electoral outcomes, Fiorina (1978, 429) judiciously concluded that "on the whole the evidence appears to indicate that economic conditions have an impact on the outcomes of congressional elections, although the specifics of the studies leave their authors wrangling about econometric techniques and their readers somewhat confused." We will attempt to alleviate some of this confusion, at least as it relates to the impact of economic circumstances on electoral outcomes during the Democratic primaries and caucuses of 1984.

The Impact of Electoral Structures

Unlike the sociodemographic characteristics of a state's electorate, the ideological disposition of its Democratic leadership and electorate, or the aggregate economic conditions pertaining in a state immediately before an election contest, electoral structures are party-specific influences on electoral behavior. Scholars as well as politicians have always been interested in the rules that structure the electoral process and in whether those rules advantage one candidate or type of candidate over others (Norrander and Smith 1985, 28; Pomper 1985, 7–8). Lengle and Shafer (1976) have argued for a truly magisterial role for party rules. They contend that "altered primary regulations, over the medium run, could well determine which blocs will be influential within a party. . . . In the long run, such ground rules may even shape the grand conflicts of an epoch, indirectly but powerfully favoring some causes and not others" (Lengle and Shafer 1976, 25, 35, 40). Other scholars, most notably Hammond (1980), have challenged Lengle and Shafer on the degree of influence that rules exercise over outcomes in the nomination process. We will ask whether any of the Democratic candidates of 1984—Mondale, Hart, or Jackson—did significantly better in open- as opposed to closed-rule states or in states that distributed delegates proportionally as opposed to states that gave some sort of bonus to plurality winners.

The Impact of Campaign Spending

The results of U.S. elections seem to suggest, if not to prove, the dramatic influence of campaign expenditures on electoral outcomes. Studies of state legislative races (Giles and Pritchard 1985; Owen and Olson 1977; Welch 1976), congressional elections (Jacobson 1984; Hinckley 1983), and state level executive races (Beyle 1983; Jewell 1984; Patterson 1982) all indicate that campaign expenditures have an impact on election results.

Nonetheless, previous analyses of the presidential nomination process have suggested a very modest impact for campaign expenditures. Goldstein (1978, 171) reports that expenditures in the 1976 primaries exhibit a "mixed pattern. . . . In 15 of the 25 cases . . . the winners spent more money than the loser, but in 10 of the cases, a full 40 percent of the total, the loser spent more than the winner." Orren's (1985, 51) early report on the 1984 events concluded that "in the primaries and caucuses of 1984 there were 26 events in which the winner outspent the loser and 26 events in which the loser outspent the winner."

Much of the evidence so far presented

has been anecdotal or, at best, bivariate considering only the direct relationship between expenditures and electoral outcomes. Although the authors cited above are undoubtedly correct to argue that outspending an opponent has not traditionally, and did not in 1984, guarantee victory, money has always been, and was in 1984, something of an advantage. We will examine the extent to which campaign spending by each of the candidates affected their electoral performance in both the primaries and the caucuses.

The Influence of Turnout

While scholars disagree about the impact of particular factors on turnout, they have demonstrated that changes in turnout work very differently in primary as opposed to caucus settings. Of the two electoral settings, the caucus results seem to be readily interpretable, while the primary results seem to many scholars to be "thoroughly perverse" (Schier 1982, 239). Schier's recent study (1982, 237) indicates that "the caucus turnout equations clearly demonstrate the explanatory and predictive utility of certain strategic variables (including what we call rules variables) to be superior to that of any demographic variables."

The factors that influence turnout in primary election settings are much less clear. Norrander and Smith (1985, 1) write that past "studies point to several factors which should influence turnout, such as the nature of the campaign, the legal structure of the primary, and the demographic characteristics of the state. However, when considering specific variables that affect turnout, these studies are as likely to disagree as agree." Ranney, in his important study of turnout in the 1976 primaries (1977, 35), concluded that "there is little of interest to report concerning the relationship between the demographic (or the economic) characteristics of the states and their turnouts in presidential primaries." We will measure the impact of turnout on electoral outcomes in both primary and caucus settings in 1984.

Simple Models of Influences on Candidate Vote Shares

We begin by presenting separate models for each of the six components of explanation for each of the three candidates (both for the primaries and the caucuses). The three major candidates in 1984—Hart, Jackson, and Mondale—represent distinct factions of the Democratic coalition, and therefore we expect the independent variables to perform differently in each candidate's model. We report separate candidate support equations for caucuses and primaries because the strategic environment is different for each type of election system.

In this study, the dependent variable is the proportion of the total vote received by each of the three major candidates in the presidential preference component of a state's primary or the first round of its caucus. We standardize the support for each candidate by taking the percentage of the total vote that he received in the state's caucus or primary.[1] Each regression equation will describe the impact of each dimension on the dependent variable in the model-proportion of the vote.

Generally, the model of the Mondale vote should represent the attributes of a candidate associated with the Democratic coalition as it was forged in the New Deal era—an organization candidate representing a coalition of have-nots battered by hard economic times. Jackson represents the major revision of the Democratic coalition made in 1964 when race became a dividing issue between Democrats and Republicans and when blacks became firmly entrenched in the Democratic camp. Hart, on the other hand, is not associated with have-nots in general or minorities in particular; rather, Hart represents the more affluent voters of neoliberal ideology who Ladd (1977; 1982) argues have transformed the Democratic party from the 1960s onward.

Sociodemographic Factors and Candidate Vote Shares

The sociodemographic groups at the core of the Democratic coalition include the poor, blacks and other nonwhites, union members and their families, Catholics and other non-Protestants, Southerners, and city dwellers in the 12 largest metropolitan areas of the nation (Axelrod 1972, 15–17; see also Abramson, Aldrich, and Rohde 1983; Petrocik 1981). Other recent research suggests that the elderly, particularly those socialized to politics during the New Deal era of the 1930s, favor the Democrats (Abramson 1975; Miller and Levitin 1976; Nie, Verba, and Petrocik 1976). Evidence also suggests that Hispanics are part of the Democratic coalition (Abramson, Aldrich, and Rohde 1983; Levy and Kramer 1973). Finally, Ladd's work (1977; 1982) indicates that a group defined as white, young, upwardly mobile professionals may be part of the Democratic party coalition. These three new groups—the elderly, the Hispanics, and the "yuppies"—will be included among the sociodemographic groups in our analysis along with the more traditional groups identified by Axelrod (1972) and others.

Our model includes a total of seven measures of the sociodemographic configuration of each state. They are: (1) proportion of the voting age population that is black, (2) proportion of the voting age population that is Hispanic, (3) labor union members as a proportion of employed persons in non-agricultural occupations, (4) Catholic, Jewish, and other non-Protestant adherents as a proportion of the total population, (5) proportion of the voting age population that is residing in urban areas, (6) proportion of the voting age population aged 65 or older, and (7) proportion of employed persons between the ages of 30 and 44 with professional or technical occupations.

The existence of a major black candidate in 1984 made the proportion of blacks in a state a central concern of each candidate. We expect Jackson's vote share to have been heavily influenced by the proportion of blacks in a state. Despite Mondale's positive evaluation within the black community, we would expect Jackson's presence to have lead Mondale to do proportionately less well in states with large numbers of black voters. Hart was clearly not associated with the black wing of the party and we would therefore expect to see Hart's vote share diminish rapidly as the proportion of black votes in a state rises.

Jackson also made a clear plea to Hispanics as part of his "rainbow coalition," and Mondale made every effort to include Hispanics in the traditional Democratic coalition of minorities. Gary Hart, whose campaign was geared toward those who were turned off by minority group influence in the Democratic party, would not normally be expected to have done well among Hispanic voters. Nonetheless, because Hart's base was in the West, his vote share might be expected to have shown a positive correlation with percent Hispanic.

Labor union voters, Roman Catholic and Jewish voters, particularly those with strong ethnic ties, and urban voters are central to the traditional Democratic coalition (Petrocik 1981). The early endorsement of Mondale by the teacher unions and the AFL-CIO was a clear confirmation of his close relationship to organized labor. We should see a larger proportion of labor union membership having advantaged Mondale and disadvantaged Hart, while Jackson should have broken even by doing well with the black labor vote. Mondale should also have fared well in the primaries and caucuses of the states with larger proportions of Catholic and Jewish voters. Hart attempted to appeal to non-core groups in the party, therefore, he should have done better in states with larger proportions of protestant voters. Jackson's campaign rhetoric and his association with Lewis Farrakan, a prominent and inflammatory black Muslim leader, weakened him among nonprotestant voters, particularly Jews. In addition, we expect Mondale and Jackson to have drawn larger proportions of the vote in heavily urban states. Hart would be expected to have done better in the states with larger proportions of rural voters.

Finally, we include age variables in our sociodemographic model of candidate vote shares. In 1984 a growing proportion of the elderly were politically aware because social security was a major issue. We expect Mondale to have done well in states with a higher proportion of those 65 and over. Hart, on the other hand, appealed explicitly to a younger generation of Democrats and Independents with his "new ideas" approach and should, therefore, have done better in states where the young are present in large numbers. The advantageous effect of the percentage of elderly and "yuppie" votes on Jackson should have been quite modest as blacks young and old would have voted for him, while whites would not have.

The results of the sociodemographic analysis are reported in Table 1. Most of the variables included in the candidate models for the primaries perform in the expected direction with the proportion of black and labor union membership being the dominant sociodemographic influences.

As expected, Hart showed his greatest strength in the primary states with less traditional Democratic constituencies. Hart territory was most clearly defined in negative terms by the absence of blacks and union voters. Viewed positively, Hart territory was white, nonunion, yuppie, and rural. Jackson was well received in states with large black populations. He did not draw significantly from states with higher numbers of Hispanic, non-Protestant, urban, or union populations, while the elderly and yuppies marginally depressed Jackson support. The Mondale vote was positively affected by the proportion of unionized voters in the state. The presence of other groups had only a modest influence on the size of the Mondale vote share.

In the caucuses we expect some differences in group support for the candidates because the participants tend to be more intensely committed to party, issues, and candidates; these are the activists as opposed to the rank-and-file voters that turn out in larger numbers in the primaries. Generally, additional sociodemographic factors are at work in the caucuses. However, with the exception of the positive relationship between yuppies and the Jackson vote and blacks and the Mondale vote, relationships are generally in the expected direction.

Political Context and Candidate Vote Shares

We include indicators for mass electorate liberalism, Democratic party elite liberalism, strength of the state Democratic party organization, and strength of the local Democratic party organization in our political context equations. These factors define the context, both ideological and partisan, that confronted the candidates when they entered a state. First, we employ an indicator of mass electorate liberalism. We selected the Wright, Erikson, and McIver (1985) measures of state ideology.[3] In 1984 the general public associated *liberal* with the New Deal, Great Society, welfare state approach to public problems. States with a large proportion of liberal voters should have provided fertile ground for Mondale and Jackson, though not for Hart.

Party leaders, however, were more likely to see Hart not as *less* liberal for not espousing traditional Democratic ideas but rather as a *different* kind of liberal. Neoliberal political analysts like Kuttner

(1985, 25) argued that "Hart . . . marrie[d] the dynamism of the entrepreneurial marketplace to the security implicitly promised by democratic citizenship. . . . The post-New Deal trick is to devise bargains that offer security without economic rigor mortis." Therefore, we include a measure of state party elite ideology in order to tap potential differences between elite and mass ideology within the Democratic party. We chose the McGregor (1978) state party liberalism score, which is constructed from state delegate preferences at previous Democratic national conventions.[4] We expect Mondale, who represented establishment welfare liberalism, and Jackson, who represented more radical implementation of these same ideas, to have fared better in states with ideologically traditional party

Table 1. The Effects of Contextual and Structural Variables on Candidate Vote Shares During the 1984 Democratic Party Nomination Campaign (Standardized Regression Coefficients)

	Primaries (N = 28)			Caucuses (N = 27)		
Variables	Hart	Jackson	Mondale	Hart	Jackson	Mondale
Demographics						
Black	−.69	+.94	−.05	−.65	+.90	+.20
Labor	−.16	+.06	+.61	.00	+.14	+.36
Urban	−.26	+.05	+.17	−.27	−.28	+.28
Hispanic	−.02	+.07	+.16	+.29	+.30	+.01
Non-Protestant	+.07	+.07	−.37	−.36	+.01	+.35
Yuppie	+.22	−.05	−.20	+.10	+.28	+.10
Elderly	+.12	−.11	−.01	−.07	+.24	+.65
R^2	.64	.92	.46	.42	.73	.55
Political context						
Mass liberalism	−.34	+.16	+.22	−.13	+.18	+.32
Party liberalism	+.44	−.50	−.15	+.48	−.59	−.17
Strength of state organization	+.13	−.33	−.06	−.07	−.08	+.02
Strength of local organization	−.04	−.08	+.32	.00	−.13	+.17
R^2	.26	.39	.19	.20	.38	.10
Economic context						
Per capita income	−.26	+.55	−.13	+.18	−.23	+.01
Change in per capita income	+.41	−.11	−.64	+.16	+.04	−.25
Unemployment	−.34	+.48	+.14	−.06	+.30	+.08
Change in unemployment	.00	+.11	+.21	+.06	+.03	+.13
R^2	.49	.45	.38	.09	.16	.08
Rules						
Closed	−.15	+.19	+.14	+.23	−.33	+.11
Delegate selection	+.25	−.05	−.53	−.07	NC	+.30
R^2	.10	.04	.32	.06	.11	.10
Spending	+.52	+.76	+.21	+.43	+.49	+.22
R^2	.27	.58	.04	.19	.23	.05
Turnout						
Turnout	NC	−.15	+.26	+.21	−.22	−.03
Change in turnout	−.17	+.38	−.17	−.13	NC	−.06
R^2	.03	.11	.05	.05	.05	.00

elites. Hart should have garnered a larger proportion of votes in states where the party elite historically has demonstrated a willingness to consider insurgent candidates.

Finally, we include indicators of strength of party organization, developed by Cotter, Gibson, Bibby, and Huckshorn (1984), in order to tap the potential impact of variations in the partisan context on the relative success of the Hart, Jackson, and Mondale candidacies.[5] Because Mondale was the clear choice of the party elite, we would expect organizational strength to have benefited Mondale at the expense of Hart and, more particularly, at the expense of Jackson.

The estimations of the models for the primaries and caucuses are reported in Table 1. The results are notably weaker than the sociodemographic equations. Nonetheless, mass liberalism performs as expected: in states with a relatively liberal tradition, Jackson and Mondale are strong, while Hart is weak. The results for party elite liberalism show that Hart's neoliberal candidacy was effective where state party delegations had a tradition of supporting outsiders. Mondale, on the other hand, did best in states that traditionally have supported mainstream establishment candidates. Jackson also does well in the traditional Democratic states, though this can largely be attributed to his strength among black voters in the South. Strength of state and local party organization had relatively little impact on candidate vote shares in either the primaries or the caucuses.

Economic Factors and Candidate Vote Shares

One of the more intractable debates within the political economy literature concerns the particular economic conditions (if there are any) that influence electoral outcomes. Specifically, is it *levels* or *changes* in such indicators as income and unemployment that cause voters to reward or punish specific candidates and parties? Disagreement, even after substantial empirical analysis, is still pervasive (Abrams 1980, 628; Crain, Deaton, and Tollison 1978; Fair 1978, 171).

Therefore, we include both level and change indicators, specifically, the state unemployment rate and per capita income and *change* in the unemployment rate and in per capita income. The levels of unemployment and per capita income are measured three months prior to the time of the caucus or primary. The economic trends are calculated as the difference between the rate twelve months before the event and three months prior to the event.[6]

Low unemployment, a decreasing rate of unemployment, high per capita income, and growth in per capita income are all signs of economic prosperity and should be positively related to Hart support. The reverse of all of these variables are signs of economic hard times and should be positively related to Mondale and Jackson support.

Estimations of the models for the primaries are reported in Table 1. The variables all perform in the expected direction with *change* in income and *level* of unemployment being the most consistent predictors. Our findings confirm that Hart did well in areas that were economically secure; where levels of unemployment were low and levels of income were high, and where rates of change in income were running strongly positive. The Jackson vote share was also profoundly affected by both levels and rates of change in economic performance. Jackson did best in states that had been hard hit by the recession of 1981–83, states in which unemployment was high and income was deteriorating. Like Jackson, Mondale ran best in states with serious economic problems, particularly those industrial states of the upper midwest in which income had declined over the previous year.

Economic factors were substantially

less powerful in influencing levels of candidate support in the caucuses. Change in income was still the most consistent predictor, working positively for Hart and negatively for Mondale and Jackson. High levels of unemployment once again provided fertile ground for the Jackson campaign. These findings for both the primaries and the caucuses strike a blow for the commonsense notion that voters are sensitive both to levels of income and employment and to changes in their rates over time.

Electoral Structures and Candidate Vote Shares

We include indicators for the degree of openness of the primary or caucus and the nature of the process used to allocate convention delegates among the candidates. State parties must decide whether their primary or caucus will be closed to nonparty members, open to some voters not previously with the party (such as independents and first-time voters), or open to all of the registered voters in the state.[7] We expect that closed events (a high score) should have increased the Mondale proportion of the vote since his appeal was directed toward party loyalists. We also expect that since blacks are overwhelmingly registered Democrats, Jackson would have done best in events closed to non-Democrats. Hart, on the other hand, should have had greater success in primaries and caucuses open to Republicans and Independents (a low score).

Rules also govern the impact electoral outcomes have on distribution of the state's convention delegates. Three general delegate distribution plans—winner-take-all; winner-take-more, bonus, or districted plans; and proportional delegate distribution schemes—have been employed by the state Democratic parties and approved by the national party.[8]

Jackson, the consistent third-place finisher, understood quite clearly that his candidacy was better off under the proportional system (high score). We expect a positive relationship between the delegate selection system indicator and Jackson support in primaries and caucuses. Hart and Mondale both made strong efforts to win votes in states with winner-take-more and winner-take-all primary rules (low score), and hence we expect a negative relationship between these procedures and proportion of vote.

The coefficients for the primaries are reported in Table 1. The models, especially those for Hart and Jackson, are very weak. The Mondale model shows that about 30% of the variance in his vote share is explained by the winner-take-all and winner-take-more rules at work in several of the primaries (Orren 1985, 36). Hart, on the other hand, did better, though only marginally so, in the winner-take-more and proportional primary states. None of the delegate selection rules shows up for Jackson, although he does, as expected, do better in the Democrat-only closed primaries.

The caucus models include some unexpected results. In the caucuses, the Mondale vote and the direct proportionality of the delegate selection procedures are positively related (although the relationship is considerably weaker than the relationship in the primaries equation). Additionally, a closed caucus is more likely to benefit Hart and to harm Jackson.

Campaign Spending and Candidate Vote Shares

We expect the share of the vote for each candidate in the primaries and caucuses to be positively related to the level of their campaign expenditures. We further anticipate that the relationship between vote share and campaign expenditures will be strongest for Hart and Jackson since they put the bulk of their campaign expenditures into state-level activities, while Mondale divided his resources between an extensive headquarters operation and spending in the states (Orren 1985). Our campaign expenditure variables measure the money spent by each of the major candidates as a proportion of all state-allocated expenditures by candidates contesting the state event.[9]

The results of our analysis highlight the fact that it takes relatively modest amounts of money to activate a receptive audience. Though Jackson had comparatively little money to spend, where he *was* able to spend it had great impact because his message mobilized voters who might otherwise have simply stayed home. For instance, fully 58% of Jackson's vote share in primary states is explained by his spending in that state; exactly half that, or 27% of Hart's vote share, is explained by his spending; but only 4% of the variance in Mondale's vote share is explained by his spending, even though he spent more money overall than Hart and Jackson combined.

Turnout and Candidate Vote Shares

We include indicators for turnout level and turnout change in the final component of our model. Turnout is measured by the percentage of Democratic supporters participating in the caucus or primary.[10] Turnout change is measured as the percentage difference in turnout between the 1980 caucus or primary and the 1984 event. Most analysts expected high turnout and turnout surge to have helped the outsider candidates, Hart and Jackson, and to have been negatively related to Mondale's vote share.

The estimations of the models for the primary and caucus cases are reported in Table 1. Neither levels of turnout nor degrees of change had a significant impact on the electoral performance of the candidates in either the primaries or the caucuses. Except for the modest advantage that increased turnout gave to Jackson in the primaries, no turnout variables explained more than 5% of the variance in candidate electoral success.

An Integrated Model of Influences on Candidate Vote Shares

The simple models of candidate vote shares provide only partial explanations, and therefore, we turn to the development and testing of an integrated model for the caucuses and primaries for each of the three candidates. We present a model in which we combine selected variables from the simple models—sociodemographic, political, economic, electoral, spending, and turnout—in a single analysis.

The sociodemographic dimension includes three variables—urban, black, and labor—that represent the core of each candidate's strength. We expect these sociodemographic factors not only to have had a direct impact on a candidate's vote share but to have influenced it indirectly through impact on the economic and political contexts. The mass liberalism and party liberalism variables are the most powerful of the political context variables and are included as indicators of that dimension. The change in income variable is employed as the indicator of economic context. Political context and economic context should have influenced vote shares directly while affecting electoral structure, campaign spending, and participation indirectly. Electoral structure, measured in terms of the openness of the primary or the caucus, is expected to have influenced vote shares directly and campaign spending and participation indirectly. Finally, campaign spending and turnout change are expected to have had a direct impact on vote shares.

Primaries

The additive models for the primaries are quite powerful, explaining nearly two-

Table 2. The Effects of Selected Contextual and Structural Variables on Candidate Vote Shares during 1984 Democratic Nomination Campaign

	Primaries (N = 28)			Caucuses (N = 27)		
Variables	Hart	Jackson	Mondale	Hart	Jackson	Mondale
Black	−.54	+.80	−.10	−.33	+.58	−.09
Labor	−.25	+.11	+.57	−.16	−.07	+.45
Urban	−.07	+.00	−.06	+.03	+.05	+.14
Change in per capita income	+.37	−.07	−.50	−.01	+.07	+.12
Mass liberalism	+.20	+.06	−.17	+.09	+.14	+.23
Party liberalism	+.13	−.06	−.27	+.19	−.01	−.36
Closed rules	−.14	+.10	+.20	+.30	−.25	+.09
Spending	+.23	+.13	−.16	+.33	+.37	+.40
Change in turnout	+.10	−.10	+.20	+.08	−.15	−.07
R^2	.82	.83	.65	.54	.81	.38

thirds of the variance in the Mondale vote and over four-fifths of the variance in both the Hart and Jackson votes. The results are reported in Table 2. The variables perform very much as expected. In this model the relative importance of each of the variables on candidate vote shares in the 28 primaries is much clearer because other relevant variables are controlled.

The integrated model for Hart in the primaries explains 82% of the variance in his proportion of the vote. Hart's vote was still from states with higher proportions of white, affluent, and nonunion voters. In addition, Hart's vote success was facilitated by heavy campaign expenditures in the states, by a liberal mass and neoliberal partisan ideological context, and by moderate increases in turnout. The integrated model explains 83% of the variance in the Jackson vote and highlights the overwhelming, even exclusive, importance of the proportion of blacks in a state to the proportion of Jackson's vote. Once we control for proportion black, other factors show a much more modest influence on the Jackson vote than the simple models suggested. The integrated model for the Mondale vote is less powerful than the models for Hart and Jackson, explaining 65% of the variance in primary voting. Mondale did best in states with a strong labor movement, a battered economy, and a history of supporting establishment candidates, in which voter turnout was high and participation was restricted to registered Democrats.

Some of the endogenous relationships (not reported here) in the path model are also interesting. In the primary states, we find a moderate explanation of mass liberalism (R^2 = .35) and of party liberalism (R^2 = .38) by the demographic variables. State demographic characteristics are less powerful predictors of the economic climate in the primary states (R^2 = .18). Nonetheless, larger black populations, larger urban populations, and smaller union membership all serve to raise the probability that a state will have a strong economic climate. Mass liberalism, state party liberalism, rules, and economic climate all affect the money available to each candidate in the states. The most consistent predictor of spending is the economic climate. In both primary and caucus states, rising per capita income had a positive effect on Hart spending and a negative effect on the Jackson and Mondale spending.

The integrated model clarifies the relative importance of the forces affecting vote proportions in the primaries. The strong performance of demographic variables in the simple models holds up in these models when other influences are taken into account. With Jackson, the candidate most clearly identified with a single demographic group, the influence continues to be almost overwhelming. Mondale's labor support is also striking. Economic climate continues to be prominent in the models for Hart and Mondale. This is indicative of the broad economic group support that each candidate represented and drew upon. This economic force is overshadowed by the importance of black support in the case of Jackson. The integrated model for the primaries demonstrates the predominance of group-defined support in the states.

Caucuses

The integrated models for the caucuses are somewhat less powerful than the models for the primaries, at least in the cases of Hart ($R^2 = .54$) and Mondale ($R^2 = .38$), though the variables in the models work generally as expected. Hart ran strong in the caucus states in which the voters were proportionally more white and nonunion and the elites were more likely to support insurgent candidates. His prospects were enhanced by spending, increases in turnout, and—somewhat surprisingly—by rules restricting participation to Democrats. Jackson found the most success in states where he was able to spend liberally in order to fully mobilize his black constituency. Once the proportion of blacks in the state's electorate and spending are considered, other factors make surprisingly little difference. Mondale's support in the caucuses came from very predictable groups under expected circumstances. Once again, Mondale's life jacket was a strong labor vote in liberal states with a history of supporting establishment candidates. He also benefited slightly by closed caucuses where he was able to spend enough to activate his adherents.

The endogenous relationships differ very little from those in the primaries. Again, the strongest relationship ($R^2 = .38$) highlights the importance of demographics in explaining party liberalism. Demographics proves to be less helpful in the caucus states than in the primary states in explaining economic climate. Again, as in the primary states, economic climate is a strong predictor of spending.

The main contribution of the integrated model to our understanding of the caucuses is to highlight the striking emergence of campaign spending. Spending, demographics, and economic climate are the most powerful predictors of candidate vote shares. However, the demographic and economic factors assume a smaller role in the case of the caucuses.

Conclusions

Clearly these integrated models tap the systematic way in which primaries and caucuses present candidates with different strategic and tactical problems. A simple way to interpret the differences between the integrated models for the primary and caucus settings is in terms of normal vote analysis. The group structure as well as the economic and ideological climates of the states are long-term effects; they are givens over which the competing candidates have very little control. They can comment on them and try to position themselves advantageously in relation to them, but they cannot change them. In the primary states, these relatively intractable long-term effects are of predominant importance. In caucus states, where participation is more restricted, the impact of group structure and ideological climate are muted, leaving a more fluid political situation in which candidate strategy and maneuver may move relatively small numbers of people with decisive effect upon the outcome. Under these more dynamic conditions, short-term influences like campaign spending and rules assume a much larger role.

Of the sociodemographic groups that previous research has identified as central to the Democratic coalition, two—blacks and labor—registered a powerful influence on outcomes in both the primaries and the caucuses in 1984. Mondale drew heavily on the support of organized labor in both settings, while Jackson drew even more heavily on his black constituency in both settings. Hart forged no close relationships with the major sociodemographic groups of the Democratic coalition. This inability or unwillingness to secure a reliable base within the Democratic electorate left Hart at a substantial disadvantage.

The impact of the mass and elite ideological climate on each of the candidates worked in interestingly consistent ways in both the primary and caucus settings. The integrated model shows that all three candidates benefitted from a liberal mass environment in the caucuses, as did Hart and Jackson in the primaries. The elite party ideological context showed that Mondale ran better in states with a history of fielding traditional liberal delegates. Hart ran better in states that had a history of bolting to candidates who do not fall into the establishment New Deal mold. Jackson's vote—once we controlled for percent black—did not respond to differences in ideological context in either setting.

Our findings also serve to shed considerable light on the long-running dispute initiated by Kramer (1971) and Stigler (1973) concerning the impact of economic performance on electoral outcomes. Our findings show quite clearly that economic performance, measured by change in income, was very important in the primaries but almost totally inconsequential in the caucuses. In the primaries Mondale did well in states with slow or no growth, and Hart did well in states that had enjoyed substantial growth. For both Hart and Mondale, economic performance was the second strongest variable in the integrated model, behind only the most powerful of the sociodemographic group influences. In the caucuses, on the other hand, Mondale and Jackson were again dependent upon their major sociodemographic support groups and upon the ideological climate, but change in income registered no appreciable impact for any of the candidates.

Campaign spending, a factor directly under the day-to-day control of the candidates and their campaign staffs, increased quite dramatically in importance in the caucus setting. In each case, spending in the caucus events assumes either the first or second place in the model; in Hart's case spending was more important than any of the sociodemographic, economic, or ideology variables; while in the Jackson and Mondale models, spending was second only to the most powerful demographic variables: blacks for Jackson and unionized labor for Mondale.

Clearly, then, the direct estimations for the primaries differ from those for the caucuses in quite dramatic and theoretically interesting ways. In sum, primaries in 1984 tended to be referenda on general economic circumstances as these impinge on broad social groups and as they were interpreted from the dominant ideological perspectives. The caucuses in 1984 tended to turn on the ability of candidates to get their central issues and themes across to their most receptive constituencies with sufficient clarity and power to move them to attend the often complex and time-consuming caucus events.

Finally, direct comparisons of the Mondale, Hart, and Jackson models provide some interesting insights into how key sociodemographic elements of the Democratic party split among these three candidates. Jackson swept the black vote, but beyond that he enjoyed only very modest support from white labor and even less support from white liberals, to say nothing of white conservatives. Similarly, the extent to which organized labor was critical for Mondale, while blacks were absent as an active component of his coalition, is quite stark. Hart, on the other hand, was dependent upon economically secure white Democrats. He enjoyed very little support from organized labor or from blacks. The results of 1984 pose a clear challenge to Democratic presidential candidates of the future: the very substantial problem of forging an appeal based on defending established Democratic interest groups, such as blacks and labor, while facilitating economic growth.

Notes

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1. The election returns for the 1984 Democratic party primaries and caucuses were drawn from various issues of *Congressional Quarterly Weekly Report* (Congressional Quarterly, Inc., Washington, DC). The best single source for both primary and caucus returns is Congressional Quarterly's *Guide to the 1984 Democratic National Convention* (1984, 36, 42) (Congressional Quarterly, Inc., Washington, DC).

2. Data for voting age population, black population, Hispanic population, population aged 65 and over, employed persons in nonagricultural occupations, population living in urban areas, and the number of employed persons between the ages of 30 and 44 with professional and technical occupations were drawn from the *1980 Census of Population and Housing* (Bureau of the Census, Washington, DC). Data on labor union membership is for 1980 (the latest year available) and was taken from the *Directory of U.S. Labor Organizations* (1982, 68, Table 21). Data on the number of non-Protestants by state is for 1980 and was found in *Churches and Church Membership in the United States 1980* (Glenmary Research Center, Atlanta, GA, 1982, 10–28, Table 3).

3. Wright et al. (1985) have created mean measures of state ideology by aggregating CBS/NYT survey responses to the question, "How would you describe your views on most political matters? Generally, do you think of yourself as liberal, moderate, or conservative?" We have reflected their weighted scores reported in their Table 3 (1985, 478–79) to produce an index with states at the high end being liberal and states at the low end being conservative.

4. McGregor (1978) classifies state party delegations to national conventions according to whether these delegations gave majority support to various liberal and conservative candidates to create cumulative indexes of state party ideology. The state Democratic party ideology indicator measures the number of conventions in which the state delegation supports the most liberal candidate. State delegations were counted as liberal if they gave majority support to Kefauver in 1952, Kennedy, Stevenson, and/or Humphrey in 1960, McCarthy and/or McGovern in 1968, and McGovern and/or Chisholm in 1972. The McGregor state party ideology indicator varies from one (most conservative) to five (most liberal).

5. To determine the strength of party organizations in the states, Cotter et al. (1984) constructed indicators of state and local party organizational strength based on survey responses from party leaders at both levels of the Democratic and Republican parties. We employed their measures as our indicators of state and local Democratic party strength. The specific indicators we used are found in Cotter et al. 1984, Table 2.4 for the state measure and Table 3.8 for the local index.

6. Data on state unemployment rates and per capita income were obtained from *Unemployment in the States and Local Areas* (1984) published by the Bureau of Labor Statistics of the U.S. Department of Labor (for unemployment data) and *Survey of Current Business* (1984), Table 1 (for personal income data).

7. Of the 27 primaries, the Democrats had eight in which any registered voter willing to declare allegiance to the party could participate, four in which only Democrats and Independents could vote, and 15 that were closed. Eighteen of the 28 caucuses allowed any voter declaring allegiance to the Democratic party to participate, while 10 permitted only enrolled Democrats to take part. No states allowed Independents to participate in the Democratic party caucuses. States allowing any voter declaring a Democratic preference to vote in the primary or participate in the caucus were coded one, those allowing both registered Democrats and Independents to take part were coded two, and states allowing only registered Democrats to participate were coded three. Data on the participation rules of the primaries and caucuses were obtained from issues of the *Congressional Quarterly Weekly Report* in late 1983 and early 1984 that contained preview articles on each of the state nomination contests.

8. Of the 51 events that determined directly or indirectly the number of delegates to be awarded to the Democratic candidates, 7 were winner-take-all contests, 10 held winner-take-more events, and 34 states used rules under which the available delegates

were divided proportionally among all of the candidates, reaching a threshold usually in the 10% to 20% range. The delegate allocation procedures were coded as follows: winner-take-all = 1, winner-take-more = 2, and proportional = 3. Data on the allocation rules used by the state Democratic parties were obtained from issues of the *Congressional Quarterly Weekly Report* in late 1983 and 1984 that contained preview articles on each of the state nominating contests.

9. Data on the campaign spending activities of the candidates contesting the Democratic party nomination were derived from the spending reports filed by each campaign with the Federal Election Commission (FEC): "Allocation of Primary Expenditures by State for a Presidential Candidate" (1985). We wish to thank Michelle Broussard of the Office of Public Records of the FEC for her assistance in obtaining the reports of the seven major candidates.

10. Data on the number of persons participating in the 1984 Democratic party primaries and caucuses were taken from *Congressional Quarterly Weekly Report*, June 2, 1984, 1317, and June 16, 1984, 1443, as well as updates reported by Orren (1985). To calculate the proportion of Democrats participating in either the primary or a caucus, we needed to derive an estimate of the number of Democratic party supporters in each state. This was accomplished as follows by using the work of David (1972) as a guide: we averaged the number of votes in each state cast in the general election for the Democratic candidates for the office of President in 1976 and 1980 and for the U.S. Senate seat and governorship in the two most recent general elections for each office. This raw number of Democratic party supporters was then used to calculate the proportion of Democratic voters turning out in either the primary or the caucus.

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THE INSTITUTIONAL FOUNDATIONS OF COMMITTEE POWER

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Legislative committees have fascinated scholars and reformers for more than a century. All acknowledge the central strategic position of committees in legislatures. The consensus, however, centers on empirical regularities and stylized facts, not on explanations. We seek to explain why committees are powerful. We formulate an institutionally rich rational-choice model of legislative politics in which the sequence of the legislative process is given special prominence. Committees, as agenda setters in their respective jurisdictions, are able to enforce many of their policy wishes not only because they originate bills but also because they get a second chance after their chamber has worked its will. This occurs at the conference stage in which the two chambers of a bicameral legislature resolve differences between versions of a bill. A theory of conference politics is offered and some evidence from recent Congresses is provided.

Legislative committees have fascinated scholars and reformers for more than a century. The early treatise writers (Bryce 1893; McConachie 1898; Wilson 1885), reformers early and modern (Bolling 1965; Norris 1945), and contemporary scholars (Eulau and McCluggage 1984; Smith and Deering 1984) all acknowledge the central strategic position of committees in legislatures. Differences of opinion concerning the role of committees persist, but there is a substantial consensus on a number of stylized facts:

- Committees are "gatekeepers" in their respective jurisdictions.
- Committees are repositories of policy expertise.
- Committees are policy incubators.
- Committees possess disproportionate control over the agenda in their policy domains.
- Committees are deferred to, and that deference is reciprocated.

There is, however, a troublesome quality to this consensus. The items in this list (and there could undoubtedly be more) describe or label committee power, but they do not explain it. Explanations of these empirical regularities require a theory. In the case of each of these stylized facts, that is, a theory is needed to determine why things are done this way. In many cases it is insufficient to refer to institutional rules because many of the practices alluded to above either are not embodied in the rules at all or have evolved from them only slowly. It is therefore necessary to begin the theoretical analysis from first principles.

There is an added advantage to a theory that begins with first principles: although formulated to accommodate some stylized facts, such a theory will

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yield new implications so that it may be employed as a discovery procedure. Consider some anomalies that the theory we formulate below can explain:

> In a bicameral system, how is it possible that change in the composition of a committee or a majority in *one* chamber is sufficient to lead to policy change (Weingast and Moran 1983)?
>
> Why are explicit procedures in the House of Representatives to diminish the gatekeeping monopoly of committees (specifically the discharge petition) rarely employed; and when they are employed, why do they rarely result in law?
>
> How is it that committees maintain their influence over policy change when, once they "open the gates" by bringing forth a proposal, majorities can work their will in ways potentially unacceptable to the proposing committee?
>
> Why do members appear to defer to committees, even to the point of defeating amendments to committee proposals that have clear majority support?

Our explanation for these stylized facts and anomalies emphasizes the enforcement of agreements and arrangements. The legislative world is one in which agreements are forged among autonomous agents. But it is a world lacking instruments or institutions that exogenously enforce such agreements (Axelrod 1981, 1984; Laver 1981). Agreements and arrangements, therefore, are subject to cheating, reneging, and dissembling. When an arrangement persists over long periods—long enough to allow students to regard it as a relatively robust empirical regularity—then either it is cheat-proof and self-enforcing, in the sense that no one has any motive to depart from the arrangement, or there exists a, sometimes subtle, *endogenous* enforcement mechanism. Although the logic of self-enforcement may apply, we believe that there is much to be learned from a theory incorporating explicit enforcement mechanisms.

In our view, the explanation of committee power resides in the rules governing the sequence of proposing, amending, and especially of vetoing in the legislative process. We demonstrate a surprisingly important role for the last stage of the legislative process, the conference procedure, in which bicameral differences are resolved. The ex post adjustment power conferred on committees in this forum provides them with subtle yet powerful means to affect the voting and proposing power of other members on the floor during the earlier legislative stages. Indeed, we show that the deference given committees on the floor is a natural consequence of the ex post adjustment powers wielded by committees in conference.

In the first section of this paper we briefly describe some alternative theoretical explanations of committee power. In each instance, we make explicit what we regard as the kernel of truth it contains, but we also point out crucial missing elements that ultimately render it incomplete. We provide the basic concepts of our own explanatory framework in the second section. In the next two sections, we develop the logic of committee enforcement emphasizing the importance of the manner in which the various stages of legislative deliberation are sequenced. In the fifth and sixth sections we provide both theoretical and empirical detail on the institutionalization of enforcement in the conference committee procedure. In the last section we pull our arguments together and address some extensions and applications.

Theoretical Foundations of Committee Power

A number of ideas exist in the traditional legislative literature about the foundations of committee power; some of these are at least a century old. A young legislative scholar in 1885, for example, characterized the veto power of congressional committees by referring to them as "dim dungeons of silence" (Wilson 1885, 69). As Bryce described it a few years later, "a bill comes before its committee with no presumption in its favour but rather as a shivering ghost stands before Minos in the nether world" (Bryce 1893, 157). At about the same time, the minority leader and soon-to-be Speaker of the House, Thomas Brackett Reed, emphasized another aspect of committee power —the advantages of information and expertise. He referred to the typical House committee as "the eye, the ear, the hand, and very often the brain of the House. Freed from the very great inconvenience of numbers, it can study a question, obtain full information, and put the proposed legislation into shape for final action" (cited in MacNeil 1963, 149). A third important aspect of committee power is proposal power. Although employed only occasionally in the very first Congresses, the practice of referring bills to a standing committee and not debating them in the full House until reported by that committee evolved during the period of the Clay speakership (1811–25). By 1825 it had become standard operating procedure in the House; and in the twentieth century, with rare exception, bills originate in committee.

Taking some liberties, then, we may describe the foundation of committee power as consisting of gatekeeping, information advantage, and proposal power. Underlying these is a system of deference and reciprocity, according to which legislators defer to committee members by granting them extraordinary and differential powers in their respective policy jurisdictions.

What is amazing about these foundations of committee power is that nowhere are they carved in granite. Committees, as an empirical matter, are veto groups that may choose to keep the gates closed on a particular bill. But parliamentary majorities have recourse to mechanisms by which to pry the gates open, the discharge petition being only the most obvious. Why, then, do parliamentary majorities only rarely resort to such mechanisms? That is, why does the system of deference to committee veto judgments survive?

The question of survival also arises concerning information advantage and proposal power. As empirical matters, these are robust regularities. Yet the Speaker of a contemporary Congress is relatively free to break any alleged monopoly of proposal power held by committees through his right of recognition in House proceedings, his referral powers, his control of the Rules Committee, and his power to create ad hoc and select committees for specific purposes. Likewise, the contributions to information and expertise from the lobbyist denizens of Washington's "K Street Corridor" and an expanded congressional staff system mitigate the alleged informational advantages of committees.

Several reasons may be put forward to explain how a cooperative system of reciprocated deference is nevertheless sustained. The first and least persuasive is that no one ever has any reason to challenge it. The committee system and its division of labor, it might be alleged, are so successful in parceling work that anyone interested in a particular subject easily obtains membership on the committee that deals with it. Under these circumstances, deference becomes self-enforcing because there are no incentives to upset the applecart. Needless to say, this explanation denies or ignores interdependence among policy areas, fiscal dependencies, and the prospect that some issues—trade, energy, and health, for example—are not amenable to a neat division-of-labor arrangement because their incidences are both substantial and pervasive.

A second, related rationale to explain deference is not so sweeping. It suggests that while the matching up of work with interested members through committee assignments is not perfect, it is nevertheless sufficient to discourage violations of reciprocity (Shepsle 1978). This view, recently popularized in more general setting by Axelrod (1984), argues that the long-term advantages of deference outweigh the occasional short-term disappointments and so serve to maintain the system.

To sum up, the argument for deference to committees claims that the benefits to be secured by violating deference and challenging a committee are either small (as in the first rationale) or not worth the costs (as in the second rationale). We believe this argument is incomplete and that its premises are not always plausible. There are, first of all, too many opportunities in which it *is* worthwhile to oppose (or to be seen to oppose) committee positions (Weingast and Marshall 1986). Second, the terms of deference to committees are extremely vague. Third, the behavioral forms violations may take range from minor opposition (say, going on record as having some doubts about a committee bill) to major revolt (introducing a "killer" amendment or initiating a discharge petition). In short, the concepts of reciprocity and deference are at best convenient terms of discourse. Their very vagueness, combined with what we believe are frequent and compelling occasions in which a legislator will not wish to honor them, greatly reduces the power of self-enforcement as an explanation of committee power.

The puzzle of committee power remains. The idea of deference as a form of self-enforcing ex ante institutional bargain among legislators cannot account for the disproportionate influence of committees in their respective jurisdictions because it cannot explain away the temptations to defect from the bargain. To be persuasive, deference must be sustained by more explicit enforcement mechanisms. We discuss three such mechanisms that committees employ to bolster their institutional influence: (1) punishment, (2) ex ante defensive behavior, and (3) ex post defensive behavior.

A committee may discourage opposition to its actions (or nonactions) by developing a reputation for punishing those who oppose it. The current chairman of the House Ways and Means Committee, a Chicago machine Democrat who knows how to keep score, was once reported to have said of a particularly obstructionist colleague, "I wouldn't support anything he wanted, even if the deal was for everlasting happiness" (personal interview). There is also the now classic story of the efforts by Senator James Buckley of New York to reduce the scale of the nefarious Omnibus Rivers and Harbors Bill. With the "help" of the Chairman of the Senate Public Works Committee, Buckley's assault on the pork barrel produced only one result—the striking of a project for the state of New York (Reeves 1974). These anecdotes aside, it would appear that the capacity to punish and the general use of a tit-for-tat strategy by the committee provide precisely the basis for the emergence of the cooperative relationship between a committee and the rest of the parent chamber so elegantly described by Axelrod (1981, 1984).

This explanation, in our view, is most convincing in the distributive politics realm in which the committee's bills are (1) of significance to a substantial number of legislators, (2) disaggregatable by legislator, and (3) introduced on a regular basis. The first condition requires that there be some prospect for punishing any given legislator in a manner that the legislator and his or her district cannot ignore—a condition not met by some highly specialized committees like Agriculture or Merchant Marines and Fisheries. The second requires that the means to punish be available so that threats are credible. The third requires that occasions to punish be readily available. For many committees, punishment of this sort is available only in blunt form, if it is available at all.

A committee may induce cooperative, deferential behavior not only by (threats of) ex post punishment but also by ex ante accommodation. Surely a committee tries, when putting a proposal together, to anticipate what will pass in the parent chamber. Similarly, it will weigh reactions to its killing a bill before actually doing so. Such anticipatory behavior, however, is hardly a basis for committee power but rather is an indication of its limitations. There are other noncommittee groups that share veto power with a committee and may use that power against committee proposals. Majorities may "veto" committee bills by voting them down. The Rules Committee in the House may refuse to grant a rule for a committee bill, thereby scuttling it. The Speaker may use his power to schedule legislation and to control debate in ways detrimental to the prospects of a committee bill. A small group of senators in the U.S. Senate may engage in filibuster and other forms of obstruction. Any individual senator may refuse unanimous consent to procedures that would expedite passage of a committee bill. In short, veto groups are pervasive in legislatures; committees are but one example. Consequently, ex ante defensive behavior by committees, necessary though it may be owing to the existence of other veto groups, cannot be regarded as an influence mechanism; rather it constitutes a recognition of the influence of others.

Having greatly qualified the significance of self-enforcing reciprocity as an explanation of committee power, we have sought more explicit enforcement mechanisms. We acknowledge a role for ex post punishment and ex ante defensive behavior. But neither strikes us as an entirely satisfactory enforcement mechanism because the conditions for the use of punishment are not met in all circumstances and ex ante defensive behavior accommodates the interests of others rather than enforcing a committee's own desires. There is, however, a third mechanism with which a committee maintains its dominance as veto group and primary policy proposer in its jurisdiction: ex post defensive behavior. We believe this to be the most potent enforcement mechanism and the least understood or appreciated.

Suppose a committee possessed an *ex post veto*. Suppose that, having molded a bill and reported it to its chamber and having allowed its chamber to "work its will," a committee could then determine whether or not to allow the bill (as amended, if amended) to become law (or, in a bicameral setting, to be transmitted to the other chamber). *The ex post veto, we assert, is sufficient to make gatekeeping and proposal power effective, even though their effectiveness appears to most observers to be the product of nothing more than informal reciprocity arrangements.*

Consider gatekeeping first. Suppose that some legislative majority could, by a discharge petition or some other bullyboy tactic, threaten to pry the gates open. If there were an ex post committee veto, then (aside from symbolic position taking) there would be little point to this sort of exercise. The ex post veto would ensure that changes in the status quo adverse to the interests of a decisive committee majority could be denied final passage. Indeed, the history of the discharge petition suggests precisely this. Even on those relatively rare occasions when a discharge petition obtained the necessary support (218 signatories), the bill of which the committee was discharged almost never became law.

Now consider proposal power. Imagine a major amendment to a committee proposal favored by a chamber majority but opposed by a committee majority. The amendment might or might not pass, but surely even its most ardent proponents would have to consider whether the amendment were distasteful enough to the committee to trigger an ex post veto. The existence of an ex post veto would encourage the amendment proponents to work out a deal in advance with the committee, would lead to a pattern in which most successful amendments were supported by a committee majority as well as a chamber majority and, in those few instances where anticipation did not discourage amendments obnoxious to the committee, would trigger such a veto.

In the remainder of this paper, we explore in an analytical fashion the ex post veto as the enforcement mechanism that allows reciprocity and deference to work smoothly. Although our model is abstract and thus is consistent with any number of different operational forms of an ex post veto, we argue that the conference procedure, in which differences in legislation between the chambers of a bicameral legislature are resolved, provides the kind of forum in which committees get a "second crack" at a bill. We believe this kind of ex post enforcement mechanism clarifies and explains why various forms of cooperation work in legislatures such as the U.S. Congress despite their transparent fragility and vulnerability.

General Framework

We employ the well-known spatial model of committee decisions, so let us here briefly review its central ingredients. The legislature consists of n *agents* ($N = \{1, \ldots, n\}$), each possessing well-defined preferences (continuous and strictly quasiconvex) over the points of an m-dimensional Euclidean space. We assume the space is partitioned into *policy jurisdictions:* X, a k-dimensional subspace of R^m, is a typical jurisdiction. Similarly, we assume N is partitioned into *committees,* with $C \subset N$ the committee whose jurisdiction is X. We shall assume that agent preferences are *separable by jurisdiction* so that we may focus exclusively on X. Thus, in X, agent i has ideal point x_i and his or her preferences are representable by strictly convex indifference contours.[1] For any $x \in X$, agent i's *preferred-to set* is defined as

$$P_i(x) = \{x' \in X | x' \succ_i x\},$$

where $\succ_i$ is i's preference relation. $P_i(x)$ is simply the convex set bounded by i's indifference curve through x; it contains all the points preferred by i to x. If D is the class of decisive majority coalitions so that $A \in D$ means $|A| > n/2$, then we may define the *win set of* x as

$$W(x) = \{x' | x' \succ_i x \text{ for all } i \in A \text{ for some } A \in D\} = \bigcup_{A \in D} \bigcap_{i \in A} P_i(x).$$

In words, $W(x)$ is the set of alternatives in X that command majority support over x. Finally, we denote a distinguished point, $x^0 \in X$, as the *status quo.*

We note in passing the best known characteristic of the spatial pure majority rule model we have just described: for almost every configuration of preferences and any $x \in X$, $W(x) \neq \emptyset$. That is, except under highly unusual circumstances, no alternative is unbeatable. This property of win sets ensures that certain sets we describe below are nonempty.

We endow the committee C in jurisdiction X with certain agenda powers. Throughout we assume that C is a *monopoly gatekeeper* in X. No change in x^0 may transpire unless C comes forth with a proposal.[2] That is, C has *ex ante veto power.* Second, C has *monopoly initiation power*: (1) changes in x^0 in jurisdiction X must first be proposed by C; but, (2) once a proposal is made by C, competing proposals (normally in the form of amendments to C's proposal) may be offered by others. Monopoly initiation power is proposal power under an *open rule.*

Finally, a committee that may withdraw one of its proposals after it has undergone modification on the floor or that is empowered to modify further or reject such proposals in some other forum (say, in a conference proceeding with its counterpart in the other chamber of a bicameral legislature) is said to possess *ex post veto power.*

In describing the various agenda powers of committees, we have in mind a specific sequence of decision making in X. Committee C may initiate the legislative process by proposing a bill to alter the status quo, x^0. Some potential proposal, x, may make a decisive committee majority worse off compared to x^0, that is, $x \notin P_c(x^0)$.[3] In this case the committee will *not* bring forth the proposal but instead will exercise its ex ante veto power by keeping the gates closed.

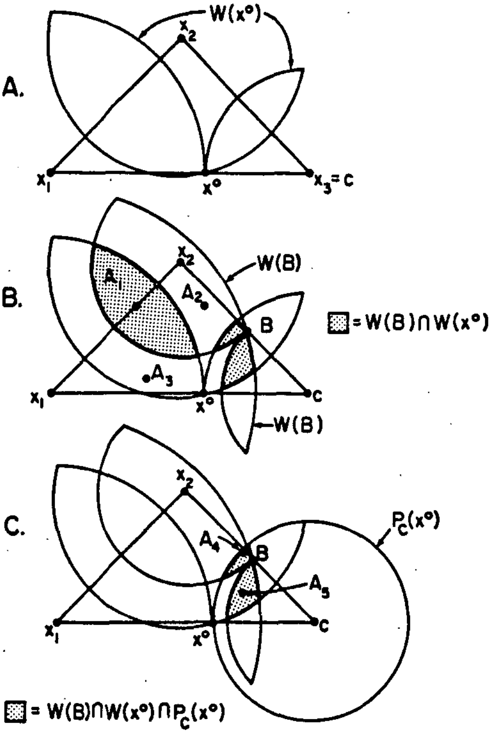
If, on the other hand, there is a proposal $x \in P_c(x^0)$, which also passes on the floor, that is, $x \in W(x^0)$, then the committee might wish to bring forth a proposal. However, it is confronted with an ambiguous prospect. Should it open the gates by proposing $x \in P_c(x^0) \cap W(x^0)$, it is entirely possible that x will be amended and that the final outcome $x' \in W(x^0)$ will have the property that $x' \notin P_c(x^0)$. Thus, by opening the gates the committee could get "rolled" on the floor and left worse off than it would have been had it kept the gates closed (Denzau and Mackay 1983). If, however, C has a second move in the sequence, that is, C has an ex post veto, then it can protect itself from getting rolled on the floor (or restore the status quo if it does get rolled) and can influence the strategic moves of agents at earlier stages of the process.

In our thinking about the institutional foundations of committee power, we place great weight on the implications of sequencing. A committee with only the power to move first—by opening the gates or keeping them closed—essentially possesses only blocking power. Once it opens the gates almost anything can happen,[4] and the committee is virtually powerless to alter the subsequent path. In contrast, a committee with powers at subsequent stages, especially the penultimate stage, not only affects the subsequent outcome but also influences the antecedent actions of others by conditioning their beliefs and expectations.

Rolling the Committee: Limitations of Gatekeeping and Initiation Power

To provide more precise intuition, we develop an example that illustrates committee power under various proposal and veto conditions. Figure 1a presents a three-person legislature, operating in a two-dimensional jurisdiction by majority rule. Agent 3 has various committee

Figure 1. Effect of Ex Post Veto



powers. The points x_1, x_2, and x_3 are agent ideal points and, to simplify the figure, we place the status quo, x^0, on the 1-3 contract locus; our argument does not depend on this feature. The set $W(x^0)$ consists of two "petals" that are composed of the points preferred to x^0 by the floor majorities $\{1,2\}$ and $\{2,3\}$, respectively.

Ex ante veto power alone is strictly a defensive tool. If x^0 lies close to x_3, then the committee can prevent subsequent change by blocking any proposal. Figure 1b has the same setup as in Figure 1a, along with various motions—B, A_1, A_2, A_3. We have also identified both $W(x^0)$ and $W(B)$. At x^0 a motion like $A_1 \in W(x^0)$ will be vetoed by the committee (thereby frustrating the preferences of a majority) since A_1 is less preferred than x^0. Should the committee bring forth a motion like B (since $B \in P_c[x^0]$), it foregoes any future influence on the course of events. Once the gates are opened with the motion B, amendments like A_1 are in order and, in this example, $A_1 \in W(B) \cap W(x^0)$. Thus, any point in the shaded region of Figure 1b, like A_1, could result since all such points defeat both B and x^0 in majority contests. (We assume here the *amendment procedure*, according to which an agenda consisting of x^0, some bill B, and amendments $A_1, \ldots, A_t$ are voted in pairwise fashion in reverse order with the losing alternative deleted.) In sum, the ex ante veto—the power to bring motions to the floor or bottle them up—is a defensive tool and, while it might be valuable to the committee because of its potential threat value, it cannot assure very much for the committee.

Joining monopoly initiation powers to the ex ante veto does not improve matters much for the committee. Initiation power allows the committee to propose points like $B \in P_c(x^0)$. But such bills, once proposed, take on a life of their own over which the committee has little subsequent control. Indeed, as we have just seen, B is vulnerable to an amendment like A_1 since A_1 can beat $B (A_1 \in W(B))$, and then it can defeat the status quo $(A_1 \in W(x^0))$. Since $A_1 \notin P_c(x^0)$, a committee proposal can lead to a decline in committee welfare precisely because the committee has no future control once it opens the gates.[5]

We have shown how limited gatekeeping and initiation power are as instruments of committee control. The former is essentially negative and the latter provides no guarantees unless expanded to proscribe all amendments to or modifications of committee proposals (closed-rule environment). Inasmuch as we rarely encounter legislatures, empirically, that prohibit modifications of committee proposals altogether, we are left with the conclusion that committee power is essentially negative. Any attempt by the committee to promote positive changes in an open-rule environment invariably results in the possibility of a decline in committee welfare. For any committee bill, B, that is, it is almost always the case that $W(B) \cap W(x^0) \neq \emptyset$ (i.e., committee bills are vulnerable to amendment); however, because $W(B) \cap W(x^0) \not\subset P_c(x^0)$, amended committee bills may leave the committee worse off. In short, once the committee opens the gates, it risks getting rolled on the floor.

Rather than jumping to the conclusion that it is inevitable that a committee will get rolled if it opens the gates, let us make some finer distinctions. It is almost always the case that no matter what proposal (B) a committee offers, there are successful modifications to it that may be offered and agents with the incentive to do so $(W(B) \cap W(x^0) \neq \emptyset$. However, such modifications need not harm the committee. In Figure 1c it may be seen that $[W(B) \cap W(x^0)] \cap P_c(x^0)$ is nonempty. The two shaded regions comprise the locus of modifications in B that both pass the legislature and leave the committee better off than with x^0. An amendment like A_4 will still be opposed by the committee (because $A_4 \notin P_c(B)$) but is nevertheless an improvement over x^0 in the committee's preferences ($A_4 \in P_c(x^0)$). An amendment like A_5 will actually be supported by the committee ($A_5 \in P_c(B) \cap P_c(x^0)$). Thus, it is entirely possible for a committee with gatekeeping and initiation powers to enhance its welfare, even in the absence of a closed rule. But there is nothing inevitable about it: while a committee might actively promote and support modifications like A_5, and ultimately accept modifications like A_4, there is nothing to prevent amendments like A_1, and there are strong incentives on the part of majority coalitions like $\{1,2\}$ to push for them.

Ex Post Veto Power

Suppose now that a committee possessed ex post veto power in addition to gatekeeping and initiation powers. Once it has opened the gates and made a proposal and after the legislature has worked its will, either accepting the proposal or modifying it in some germane fashion, the committee now may either sanction the final product or restore the status quo, x^0. A committee with an ex post veto possesses the power to protect itself against welfare-reducing changes in the status quo. The ex post veto shares with the ex ante veto this defensive property. But because of its position in the sequence of decision making, the ex post veto confers offensive capabilities as well. Coming last in the sequence, it affects prior beliefs and behavior of other agents.

In Figure 1b suppose that committee bill B stimulates the floor amendment A_1. As noted earlier, $A_1 \in W(B) \cap W(x^0)$, with Member 1 and Member 2 preferring it both to B and to x^0. However, $A_1 \notin P_c(x^0)$ so that, with the ex post veto, if A_1 passes, then the committee will veto it and reinstate x^0. A vote for A_1, then, is in reality a vote to maintain the status quo. But both members 2 and 3 prefer B to x^0. Thus, despite a nominal preference for A_1 over B, Member 2 finds the prospect of an ex post veto a credible threat and joins with Member 3 in defeating all amendments like A_1. In short, while an agent like 1 has every incentive to move an amendment like A_1 against B, sophisticated calculation induced by ex post veto power leads Member 2 to depart from a nominal preference for A_1 and vote against it.

The ex post veto ensures that the final outcome will either be x^0 or an element of $P_c(x^0)$. It therefore protects a committee from being rolled on the floor. One would expect, as a consequence of ex post veto power, that many amendments nominally supported by legislative majorities will not pass on the floor if they are opposed by a committee majority. Such is the case for all amendments in the one shaded petal of Figure 1b containing A_1. Opposed by the committee, such amendments will be voted down by sophisticated majorities.

The ex post veto does not protect against amendments in the shaded regions containing A_4 and A_5 (see Figure 1c) because the veto threat is no longer credible there. In these instances, the final outcome is still superior to x^0 in the committee's preferences. The committee may bluster, but it will not veto. Thus, some amendments (like A_4) will pass despite committee opposition, and others (like A_5) will pass with committee support. These amendments turn out to be nonproblematical for committees, as we show in the next section.

There is one aspect of behavior induced by credible threats of ex post veto (such as the case of A_1) that bears further discussion. As we related in the introductory section, much is made in the congressional literature of a system of reciprocated deference. But why is deference practiced at all? Is deference unqualified and honored always and everywhere? Our predictions provide a more discriminating explanation of this aspect of deference (or the appearance of it) than does the more traditional lore. In the case of an amendment like A_1, Member 2 may *appear* to defer to the committee by voting against the amendment despite a sincere preference for it; indeed, Member 2 may rationalize his or her own behavior in this way. Thus, one might wish to *label* this behavior *deference. But it should be clear that it is deference to the ex post veto power of the committee, not deference to expertise or an instance of reciprocal cooperation.* In the absence of an ex post veto, we would not always expect to see deference by Member 2; rather, if the committee opened the gates in the first place, we would expect to see members 1 and 2 support an amendment like A_1. *Likewise, even with an ex post veto, there are some amendments to a committee bill (even some opposed by the committee) for which no deference at all will be observed.* An amendment like A_4, for example, will find majority support and no deference because the veto threat is not credible here. In our view deference is endogenous, is not everywhere applicable, and is most usefully thought of as a reflection of the strategic character of a situation. It is a property of a sequential equilibrium (Kreps and Wilson 1982).

In this light, the anomaly begging for explanation is not Member 2's counterintuitive, seemingly deferential behavior but rather why motions like A_1 are ever made in the first place. We can allude to the symbolic position taking of Member 1 in moving A_1, but this is surely not a very deep explanation. A more promising view incorporates the fact that agents, like Member 2, may not always be in a position to vote strategically (Denzau, Riker, and Shepsle 1985). In moving A_1, Member 1 seeks to defeat *B* with a "killer amendment" that he or she knows will ultimately trigger a veto and the reinstatement of x^0. Member 1 exploits Member 2's inability to cast a strategic (read: "deferential") vote. If Member 2 is not disabled in this way, then when A_1 is moved and defeated, we believe strategic recognition of the ex post veto is an explanation superior to arguments about deference.

The discharge petition may be thought of in very similar terms. Suppose the original bill were A_1 and assigned to the committee of Figure 1b. Clearly, the committee's disposition is to keep the gates closed and not report A_1. Since $A_1 \in W(x^0)$, a majority has an incentive to discharge the committee of its jurisdiction over this bill. Why, then, is the discharge mechanism rarely resorted to? And when it is employed, why does it rarely result in law? The ex post veto provides an explanation. Discharge petitions are often not worth the effort because of the strategic realities. While they get around the ex ante veto, they do not affect the ex post veto. So long as the committee gets to take a crack at the bill *after* its chamber has worked its will, it is in a strong position to affect the course of its chamber's deliberations. Once again, it is strategic calculation, not deference, that provides the more compelling explanation.[6]

Institutionalization of the Ex Post Veto: Conference Committees

In the United States Congress, as in most state legislatures, a bill must pass both chambers of the legislature in precisely the same form before it may be sent to the chief executive for his signature. Should a bill pass in different forms in the two chambers, a process is set in motion to reconcile differences.[7] After the second chamber has acted on a bill, the first chamber may "concur" in the second chamber's amendments. If, instead, the first chamber "disagrees" with the second chamber's amendments (or concurs in those amendments with further amendments of its own), then the second chamber may "recede" from its original amendments (or concur in the first chamber's new amendments). Or it may, in turn, concur in the first chamber's new amendments with its own new amendments, putting the ball back in the first chamber's court. Although this process, known as *messaging between the chambers*, cannot continue indefinitely, the bill can be sent back and forth several more times in the hope that one of the chambers will accept the final position of the other. However, once a stage of disagreement is reached in which one chamber "insists" on its version of the bill and the other chamber disagrees, then one chamber requests a conference, and the other chamber accepts. While as many as three-fourths of all public laws manage to avoid the conference stage, nearly all major bills—appropriations, revenue, and important authorizations—end up in conference.

There is now a considerable body of rules and commentary on conference proceedings.[8] Conferees of each chamber (also called *managers*) are appointed by the presiding officer; these appointments come principally from the committees of jurisdiction at the suggestion of those committees' chairpersons (some evidence is provided below). Occasionally an additional conferee is appointed to represent a particular amendment that the presiding officer (in the House) believes will not otherwise be fairly represented (like A_1 in Figure 1); but even in this exceptional case, the views of the committee chairpersons are dominant.[9] The conferees from each chamber seek to resolve differences in the respective versions of the bill, and an agreement is said to be reached when a majority of *each* delegation signs the conference report.[10] If both sign, the report and accompanying bill containing the agreement are brought back to each chamber to be voted up or down (no amendments are in order). That is, the conference report is considered under a closed rule as a take-it-or-leave-it proposal.[11]

The conference procedure, described in simplified fashion in the preceding two paragraphs, thus does two things. First, it institutionalizes the ex post veto and, as described in the previous sections, gives credibility to the committee during floor deliberations in its chamber. Second, to the extent that there is some discretion on the part of conferees on the terms to which they may agree (see below), the take-it-or-leave-it treatment of conference reports confers additional ex post adjustment power on the committee. It is to this latter consideration that we now proceed.

We begin with the jurisdiction X, which we assume is common to both the House and the Senate, and the status quo $x^0 \in X$. Four sets in X are of interest:

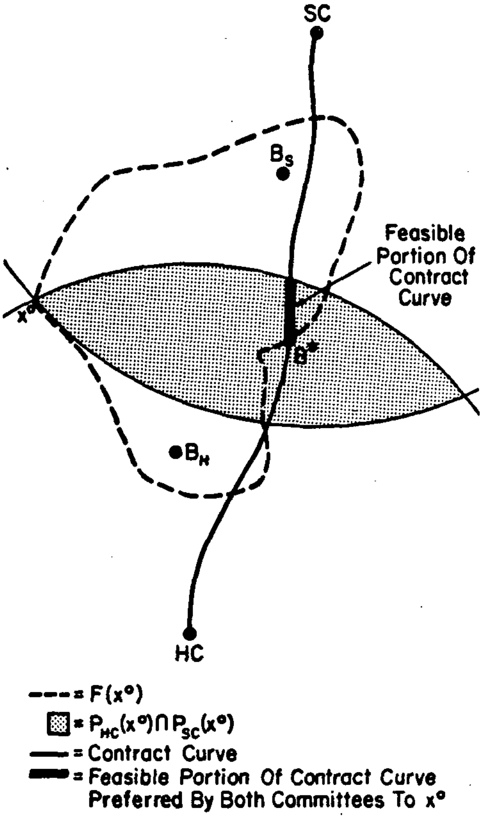
1. $W_H(x^0)$: win set of x^0 in House
2. $W_S(x^0)$: win set of x^0 in Senate
3. $P_H(x^0)$: preferred-to set of x^0 of House committee
4. $P_S(x^0)$: preferred-to set of x^0 of Senate committee

We have already seen from Figure 1 that the final outcome must be an element of $W_H(x^0)$. House majorities constrain changes in x^0. Likewise, in the Senate $W_S(x^0)$ is a constraint set. To pass, therefore, the conference outcome must be an element of $F(x^0) = W_H(x^0) \cap W_S(x^0)$. The status quo, x^0, may be imposed by either conference delegation (which we assume to be the relevant legislative committee in each chamber) if a proposed settlement is not an element of one of their preferred-to sets, that is, $P_H(x^0)$ or $P_S(x^0)$, respectively. Thus, we have as a necessary condition for a change in x^0 the following set inequality:

$$F(x^0) \cap P_H(x^0) \cap P_S(x^0) \neq \emptyset.$$

The ex post veto power of a committee follows from the fact that it represents its chamber in conference proceedings and may refuse to agree to a conference settlement. If the preferences for change of the House and Senate committees, for instance, fail to intersect ($P_H(x^0) \cap P_S(x^0) = \emptyset$), then any proposed change will be

Figure 2. Conference Committee Bargaining


SC
B_S
Feasible Portion Of Contract Curve
x^0
B^*
B_H
HC
---- = F(x°)
▨ = $P_{HC}(x^0) \cap P_{SC}(x^0)$
—— = Contract Curve
▬ = Feasible Portion Of Contract Curve Preferred By Both Committees To x°

vetoed by one of them. Similarly, if changes preferred in common by the two delegations fail to intersect the feasible set, $F(x^0)$, then no alteration of x^0 is possible.

We are not yet in a position to model conference proceedings explicitly, but on the basis of the set inequality above, there are some additional points to be made about the opportunities the conference mechanism presents to committees. Assume, then, that the set inequality holds. In Figure 2 we depict x^0, $F(x^0)$, House and Senate committee ideal points (*HC* and *SC*, respectively), and the committee contract locus ($P_H[x] \cap P_S[x] = \emptyset$). The latter curve is heavily shaded where it has common intersection with $F(x^0) \cap P_H(x^0) \cap P_S(x^0)$. Also, we have indicated the bills, B_H and B_S, that have passed the respective chambers in different forms, necessitating the conference.[12]

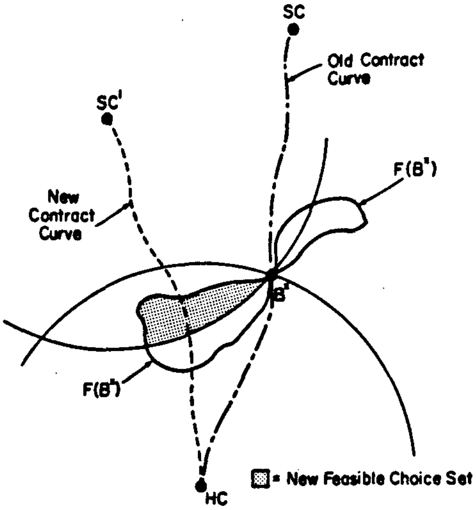
The respective conferees take $F(x^0)$ as a constraint and seek a negotiated settlement, say B^*, consistent with that constraint. This normally requires a compromise in which the preferences of each chamber (as reflected in B_H and B_S respectively) are to some degree sacrificed. Indeed, in Figure 2, the committees sacrifice as well, agreeing on an outcome less preferred to them than their respective chamber's bills. Different configurations of preferences, however, need not have this property.

In the empirical literature on conference committees, much is made of who "wins" in conference (Fenno 1966; Ferejohn 1975; Strom and Rundquist 1977; Vogler 1970). Sometimes the outcome is closer to the House position, sometimes closer to the Senate position, and sometimes it entails splitting the differences between the chamber positions. From Figure 2 it is clear that such outcomes cannot be attributed entirely to relative bargaining skills or to which chamber acted first (explanations common in the literature). The nonconvexity of $F(x^0)$ means that some compromises are infeasible (they may lie outside $F[x^0]$). Moreover, the ultimate compromise, B^*, may well lie closer to one bill than to the other, or closer to one committee's ideal than to the other's. But once again this cannot be attributed entirely to relative bargaining advantages since the relative locations of $F(x^0)$, $P_H(x^0)$, and $P_S(x^0)$ will restrict the feasible set of agreements. In Figure 2, B^* is about equidistant from the Senate committee's and the House committee's ideal. But it constitutes the best the House committee could hope for, given the constraints, whereas the Senate might have done better. In general, the configuration of chamber preferences and of conferee preferences will determine the feasible bargaining range.

While the task of modeling conference proceedings falls under the rubric of future research, there are two more insights we can offer, one on equilibrium and the other on comparative statics. In Figure 3, B^* is the negotiated conference agreement (from Figure 2) between *HC* and *SC*. It lies on their contract locus and is an element of $F(x^0)$ (not pictured). There is no reason, however, to believe that chamber majorities are content with B^*. Since $B^* \in F(x^0)$ and since it comes back to each chamber under a closed rule, it will pass. But it is entirely possible that $F(B^*) \neq \emptyset$, as shown in Figure 3. Nonetheless, in these circumstances B^* is now an equilibrium. Since $P_H(B^*) \cap P_S(B^*) = \emptyset$ (i.e., B^* is on the *HC*–*SC* contract locus), the set inequality is violated so that despite clamoring from both chambers for change, none will be forthcoming. For every proposed change in B^*, at least one of the conference delegations will exercise its ex post veto. Any such proposal will die in conference.

Our model also yields important comparative statics results. An equilibrium point, that is, a status quo point for which the set inequality is violated, will be upset by exogenous changes in committee composition (but not by changes in *chamber* composition not affecting committee composition as well). In Figure 3, if the Senate committee's ideal shifts from *SC* to *SC′*, an entirely new contract locus is traced out, and B^* is no longer in equilibrium. This suggests two nonobvious comparative statics implications. First, the ex ante and ex post vetoes of committees may neutralize even dramatic changes in chamber composition, slowing if not blunting altogether the tracking of policy with popular preferences. Second, committee composition changes, even if restricted to only one chamber, have a disequilibrating effect. Thus, as Weingast and Moran (1983) discovered about the Federal Trade Commission, dramatic changes in the composition of the Senate oversight committee (with no concomitant changes on the House side) in the 1970s were sufficient to set into motion a major change in policy direction at the Federal Trade Commission.

Figure 3. Conference Committee Comparative Statics



Committee Dominance of the Conference

In order for committees of jurisdiction to possess an ex post veto, they must dominate conference committee delegations. On the basis of the reports of early students of the subject (unfortunately, without much in the way of supporting evidence), such dominance has been the case for more than a century (Rogers 1922; McCown 1927). We do not present a full-blown empirical analysis here, but in order to give some veracity to our claims we have examined all conferences listed in the Congressional Information Service's Annual Abstracts of Congres-

Table 1. Conference Committee Composition

	Number of Conferees Not on Germane Committee					
	1981		1982		1983	
Type of Conference	House	Senate	House	Senate	House	Senate
Budget[a]	0 (15)	0 (12)	0 (15)	0 (10)	0 (15)	0 (8)
	[2]		[3]		[1]	
Appropriations[b]	0 (168)	0 (187)	0 (155)	0 (187)	0 (225)	1 (219)
	[12]		[11]		[8]	
All others	2 (190)	3 (155)	3 (399)	3 (276)	2 (206)	0 (126)
	[17]		[38]		[16]	

Source: Congressional Index Service, 1981–83.

Note: Cell entries give total number of conferees *not* from the committee of jurisdiction for each conference type. In parentheses are the total number of conferees. In brackets are the number of conferences.

[a]Neither the Omnibus Budget Reconciliation Act of 1981 nor the Omnibus Budget Reconciliation Act of 1982 is included here. Each was an exceptional situation involving an unusually large number of conferees.

[b]Does not include omnibus supplemental appropriations or continuing resolutions.

sional Records and Legislative Histories for 1981, 1982, and 1983 (Congressional Information Service, Bethesda, MD).

Before reporting our evidence we note that a consequence of the 1970s reforms in the House and of the loose germaneness restrictions in the Senate is that many pieces of legislation are the handiwork of several committees in each chamber. A House bill, for example, might be amended in a nongermane manner by the Senate. Conferees are drawn from the committees of original jurisdiction plus additional conferees to deal (only) with the nongermane Senate amendment.[13] Alternatively, it is occasionally the case in the House that the Speaker partitions a bill into parts and commits these to different committees for hearings and markup according to their respective jurisdictions; in the Senate, multiple referral may occur by unanimous consent. Again, conferees from all relevant committees make up the delegation.[14]

In Table 1 we present evidence on conference committee composition for the conferences held from 1981 through 1983. For each year, by chamber and type of legislation, we report the number of conferees who were *not* members of the committee(s) of jurisdiction. The data are crystal clear in their message. On only one occasion in the three years, was a member—not sitting on the Appropriations Committee of either chamber—a conferee for an appropriations bill. On only a handful of occasions (fewer than 1% in the House; about 1% in the Senate) were noncommittee members conferees for legislative committee bills. And finally, on budget resolutions only members of the two budget committees were conferees.

A further perusal of the data on which Table 1 is based yields additional impressions, though we will not attach any quantitative weight to them here. First, it is almost always the case that the chairperson and the ranking minority member of the full committee from which the bill originated serve on the conference. Second, it is extremely rare for a conference to produce an agreement to which these two persons are not signatories; it

Table 2. Subcommittee Autonomy in Conference: Appropriations Committees

	Senate Autonomy		
House Autonomy	Complete	Dominant	Partial
Complete	18	3	2
Dominant	2	1	0
Partial	0	0	0

Key:

Complete: The conference delegation was identical to the subcommittee membership.

Dominant: Either one subcommittee member was deleted, or one nonsubcommittee member was added to the conference delegation.

Partial: Subcommittee representation was neither complete nor dominant.

Note: Table includes Appropriations conferences in 1981, 1982, and 1983 exclusive of omnibus supplemental appropriations or continuing resolutions.

happens on occasion (for example, Chairman Hatfield did not sign several Appropriations conference reports), but we hesitate to draw any conclusions from these events for they are likely to involve contextual details that are not available without in-depth study of the particular cases. Third, there is considerable evidence that, in addition to full committee chair and ranking minority member, the subcommittees responsible for the bill dominate the conference delegation (see below for some additional details).

Committee dominance at the conference stage is perhaps the most complete and certainly the most obvious in our data in the area of appropriations. Moreover, the decentralization to the subcommittee level within each appropriations committee that Fenno (1966) described twenty years ago is clearly evident at the conference stage as well. In Table 2 we display the evidence for this claim for all appropriations measures (omnibus bills excepted) in 1981, 1982, and 1983. Subcommittee autonomy is said to be complete in conference if all the members (and only all the members) serve as managers. Subcommittees are dominant when either one subcommittee member was excluded from the conference or a nonsubcommittee member was included. Since the former circumstance may often arise with no political weight attached (e.g., a Senator is out of town; a Representative is ill) and the latter occurred on only a single occasion, most of the dominant autonomy occurrences are hardly different from their complete autonomy counterparts. Finally, partial autonomy arises when more than one subcommittee member is deleted from conference. As the evidence suggests, subcommittees of both appropriations committees not only take full responsibility within their respective chambers for marking up appropriations measures and managing them on the floor but the same (relatively small) group of legislators meets year after year to hammer out a final compromise.

As a final bit of empirical corroboration, we have taken a sample of conferences by legislative committees from the 1981–83 period to see the extent to which the subcommittee autonomy evidenced in the appropriations realm carries over to other types of legislation. The results appear in Table 3. Of the 71 legislative committee conferences from the 1981–83 period, we examined the composition of 27 to see the extent to which the subcommittee of jurisdiction dominated the conference delegation. The evidence of subcommittee influence here, while not as overwhelming as in the appropriations realm, is nevertheless con-

Table 3. Subcommittee Autonomy in Conference: Legislative Committees

House and Subcommittee Membership	On Conference	Off Conference
House Members		
On subcommittee	210	248
Off subcommittee	25	720
Senators		
On subcommittee	136	72
Off subcommittee	35	204

Note: The populations are the committees of jurisdiction. The first row of each panel gives the number of subcommittee members on and off the conference delegation. The second row gives the number of non-subcommittee members (but on the full committee) on and off the conference delegation.

siderable. In both chambers subcommittee members dominate the conference delegations. In the House they constitute about 90% of the conferees; in the Senate, nearly 80% of the conferees. More importantly, the median case is one in which the conference delegation is drawn entirely from the subcommittee of jurisdiction.

Discussion

We have sought to offer a more discriminating notion of committee veto power, to embed in in a decision making sequence, and thereby to provide a firmer explanatory foundation for committee power than has been provided heretofore. Our theoretical examples and the accompanying figures illustrate the methodological tools and suggest the lines of what is a fairly general argument. Of central importance is the role of sequence. It matters, for example, whether veto power comes first (as in gatekeeping) or at the penultimate stage (as in conference proceedings). An undiscriminating treatment of committee agenda power that fails to distinguish between different sequential properties of that power is often misleading.

In emphasizing sequence and explicit enforcement arrangements, we do not intend to deprecate the ideas of self-enforcing agreements, implicit cooperation, and deference that have constituted traditional stock-in-trade explanations for committee power. Surely, all of these operate. Moreover, our focus on ex post enforcement is in no way inconsistent with the fact that many participants might themselves explain their behavior as essentially deferential. It would not surprise us to find most legislators saying, "Sure, I let those people over on Education and Labor do pretty much what they think is reasonable. And they do the same for us on Armed Services. That's the way things are done around here." We would only claim that "deference" labels a behavioral regularity; it does not explain it. The theoretical question of interest is why that behavior is an equilibrium. We have, in effect, sought to give deference a rational basis by embedding it in the strategic realities produced by the sequence of decision making.[15]

Much work, both theoretical and empirical, remains to be done. In the body of this paper, we have only hinted at the broader generality of our argument. A first-order priority is to specify theoretical conditions more explicitly and generally. Second, we need to understand committee strategies better. What is the optimal markup vehicle that a committee takes to the floor (see Shepsle and Weingast 1981)? What amendments will committee members themselves seek to offer on the floor?

To what extent do committees (party leaders, backbenchers) anticipate the conference stage, and how do these expectations and forecasts affect their prior floor behavior? Third, we have given little attention to the strategic opportunities available to noncommittee members. Given the partial control by committees, what strategies may noncommittee members pursue to influence committee legislation? Finally, how might we properly model the conference itself, the objectives of the participants, and the constraints imposed upon them? These are all theoretical questions upon which our methodology may be brought to bear.

Empirically, there is a good deal of qualitative description and quantitative work in the legislative literature on some aspects of the problems we have presented in this paper. Most of it, however, is not tied to a theoretical framework; and, as we pointed out earlier, it is not at all obvious to us that the main preoccupation of this literature—namely, the question, who wins in conference?—is at all illuminating. Conferees are constrained by what will pass their respective chambers and this in turn determines the feasible set of agreements conferees may reach. The evidence presented in the previous section on committee (subcommittee) autonomy suggests an even more persuasive reason for doubting the relevance of this question. The conference may be less an arena for bicameral conflict than one in which kindred spirits from the two chambers get together to hammer out a mutually acceptable deal. Surely on some (many?) subjects—for example, commodity price supports—the members of the House and Senate (sub)committees who control the conference have more in common with one another than either may have with fellow chamber members.

In our analytical approach to legislative institutions, we have focused on the locus and sequence of agenda power. In characterizing legislative decision making in terms of who may make proposals (motions, amendments), and in what order, and who may exercise veto power and in what order, we wish to emphasize that these features are not merely the minutiae of parliamentarians. Rather, they provide the building blocks from which legislative institutions are constructed. The results presented here and by others elsewhere show that different mixes of these institutional building blocks lead to different outcomes and, correspondingly, to significantly different political behavior.

In the context of the committee system in the U.S. Congress, we showed that proposal power and ex ante veto power are insufficient to the task of institutionalizing an effective division-of-labor arrangement. In the absence of some form of ex post veto power, committee proposals are vulnerable to alteration and, because of this, committees have agenda control in only a very truncated form. It is unlikely, in our view, that such a shaky foundation would induce individuals to invest institutional careers in the committees on which they serve.

Although our analysis focused on the U.S. Congress and the manner in which the ex post veto is institutionalized there, it should be clear that our approach is more general. Because it can, in principle, be used to study any sequence of agenda control, it can be applied to institutions that differ significantly from Congress. Thus, we would conjecture more generally that bicameral legislatures in which committees are not the central actors in resolving differences between the chambers will not possess strong committees, ceteris paribus.

It is in this regard that the British Parliament is of some interest. The method of resolving differences between two chambers of a bicameral legislature is of British invention. The earliest recorded evidence of its practice comes from fourteenth century England. But in England, as Rogers (1922, 301–2) observes,

> It had fallen into desuetude even before the Parliament Act of 1911 so attenuated the powers of the House of Lords. Controversies between the two chambers are not serious, or, except in rare instances prolonged. . . . Since the Government stands sponsor for practically all legislation, a conference between the Ministers and leading Peers in Opposition is able to compose the differences and, indeed, ministerial responsibility is ordinarily sufficient to prevent conflicts between the chambers or the necessity for a conference.

The institutions of cabinet government reduce the need for representatives of the two chambers to meet in conference to resolve differences. The centralized leadership of the cabinet confers agenda power in *both* chambers on the same single group of ministers. They possess proposal power and they control (either explicitly or through bargaining) the amendment process. There is no need for ex post reconciliation since the cabinet may choose policies that will survive both chambers ex ante.

A detailed application of our approach to this institution is beyond the scope of this paper. However, the outline above suggests three implications. First, centralized agenda power in the Parliament implies that policy across different areas is likely to be more coordinated than in the committee-based Congress. Because the committee system in the Congress delegates agenda power area by area to different individuals with not necessarily compatible goals, coordination across policy areas is more difficult. Second, the Speaker in the House of Representatives is structurally disadvantaged in comparison to the Prime Minister in Parliament. Because the Speaker holds few of the critical elements of agenda power, he must depend extensively on persuasion to induce others to pursue his own objectives. On the other hand, the Prime Minister holds important powers over the ministers because they owe their positions to the Prime Minister rather than to an independent property rights system conveyed by seniority.[16] Third, we conjecture that, because of the cabinet institution, a system of standing committees in the British Parliament would most likely lack the sort of ex post veto with which congressional committees are blessed. By the argument of this paper it would be surprising if a full-blown committee system of the U.S. type were ever to develop. This is but another way of saying that institutions of ex post enforcement confer power on committees. In their absence, we doubt committees would play the consequential role they do in the U.S. Congress.

Notes

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1. The assumption of jurisdictional separability does not preclude inseparabilities within jurisdictions. In most of our examples, we draw indifference curves as circles, but our arguments extend more generally to any convex level sets.

2. In later discussion, we relax this requirement in order to determine what might happen if other legislators have the capacity to pry open the gates.

3. For committee C, by $P_c(x)$ we shall mean the set of points preferred to x by a *decisive committee majority*. We do not here dwell on the characteristics of such decisive sets.

4. Formal rules governing amendments and requirements like voting the status quo last do place restrictions on final outcomes. Nevertheless, this set typically contains a range of alternatives (some not in $P_c(x^0)$) over which the committee is unable to exercise subsequent control. See Shepsle and Weingast 1984.

5. We assume here and throughout the paper that all agents vote sophisticatedly (Farquharson 1969) and are able to anticipate the strategic moves

of others. Thus, amendments to B like A_2 and A_3 will fail. The former actually beats B ($A_2 \in W[B]$). But since it would subsequently lose to x^0 ($A_2 \notin W[x^0]$), voters 2 and 3 vote against it because they prefer B to x^0 (even though 2 prefers A_2 to B). A_3 fails even though a majority prefers it to x^0 since $A_3 \notin W(B)$.

6. Notice that our explanation does not assert that discharge petitions will never be used or never work, only that they are unlikely to be used or used successfully for bills like A_1 or A_3. If, for whatever reasons, the gates were kept closed on a bill like A_4, a discharge petition *would* work because the threat of an ex post veto is incredible. Moreover, the discharge petition may be used strategically in this case to force a committee to report a bill in a form or at a time that is less than ideal from the committee's perspective.

7. What follows is based on the masterful essay by Bach (1984) on conference procedures.

8. The interested reader may consult the rules of the House and Senate, the *Senate Manual, Deschler's Procedure in the House of Representatives,* and *Jefferson's Manual.* At the turn of the century, the House and Senate codified procedures. The codification, conducted by Thomas P. Cleaves, clerk of the Senate Appropriations Committee, bears his name and is known as *Cleaves' Manual of Conferences and Conference Reports.*

9. Dr. Stanley Bach of the Congressional Reference Service informs us that while the presiding officer in each chamber *appoints* the conferees, their *selection* differs in each chamber. In the Senate, appointment is by unanimous consent so that the presiding officer exercises no discretion in selection; in practice, he proposes the list submitted by the chairperson of the committee managing the bill. In practice, the Speaker of the House does the same. But the standing rules in this chamber give the Speaker discretion (and instruction) so that the final outcome is the result of bargaining between the Speaker and the relevant committee chairperson.

10. Thus, even if a proponent of an amendment opposed by the committee is a member of the conference delegation, his or her views are decidedly in the minority.

11. Technically, the first chamber to vote on a conference report has a third option—to recommit it to conference (sometimes with nonbinding instructions). Given the press of business, this often has the same effect as voting the report down. If the first chamber does pass the report, then the second chamber is left to take it or leave it (since the conference committee is disbanded after the first chamber's action). Finally, as a technical matter, if either an ex post veto is exercised or one of the chambers defeats a conference report, the bill is not necessarily dead since the chambers may return to messaging or may appoint new conferees. Again, however, the practical effect is to harm, often irreparably, the search for a House-Senate reconciliation.

12. In the figure, both bills are elements of $F(x^0)$, that is, either is preferred to x^0 by *both* chambers. Of course, this need not be the case inasmuch as the common circumstance is one in which one bill, say B_H, could not pass in the other chamber. The particular depiction in Figure 2 does not affect the points we make in the text.

13. An instance of this (and there are many) occurred in the Cash Discount Act of 1981, a bill managed by the House and Senate Banking Committees. The principal managers for each chamber were drawn from these committees. But one part of the bill (section 303) fell into the jurisdiction of Energy and Commerce on the House side and Labor and Human Resources on the Senate side. Additional conferees from these two panels were appointed to resolve differences in this section of the bill.

14. Thus, the Department of Defense Authorization Act of 1982 was marked up principally by the House Armed Services Committee, but sections of it were considered by the Select Committee on Intelligence and the Committee on Judiciary. Each of these panels was represented on the conference delegation with specific responsibility for those sections of the bill falling in their jurisdiction.

15. Throughout we have emphasized explicit enforcement institutions as the glue holding agreements together. In the story we tell about conference committees, however, there is a key feature for which we cite no explicit enforcement: Why does the Speaker, in his discretion, appoint committee members to the conference delegation? Without this move on the Speaker's part, committees would lose their post-floor deliberation role. Space precludes an extensive discussion here, but we think the Speaker's observance of this norm is a key to understanding what leaders must do to stay in office.

16. In this sense, there is an apt parallel between the agenda powers of a contemporary Prime Minister and a turn-of-the-century Speaker of the House.

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PARTICIPATION AND PURPOSE IN COMMITTEE DECISION MAKING

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Participation in committee decision making is an important form of legislative behavior but one we know little about. I develop a model of committee participation and test it using data drawn from staff interviews and records of the House Committee on Education and Labor. The analysis confirms that congressmen are purposive actors, but it also shows that different interests incite participation on different issues and that motivational effects vary in predictable ways across legislative contexts. If members are purposive, however, they also face a variable set of opportunities and constraints that structure their ability to act. Members and especially leaders of the reporting subcommittee, for instance, enjoy advantages in terms of information, staff, and lines of political communication. At the same time, freshman status entails behavioral constraints despite the reputed demise of apprenticeship in legislative life. Understanding such patterns of interest and ability, I conclude, should permit us to illuminate several larger questions regarding decision making and representation in a decentralized Congress.

While committees are crucial to the legislative process in Congress, participation in committee decision making is not universal. Over two decades ago, Charles Clapp (1963, 265–66) observed that less than half a committee's members regularly participated in its deliberations. More recent accounts suggest that truancy at committee meetings is severe—a general response to the growing congressional workload and the proliferation of congressional workgroups (Davidson 1981, 109–111; Dodd and Oppenheimer 1985, 46). On any given bill, the membership of a committee or subcommittee abdicates its considerable legislative authority to some subset of self-selected members.

Viewed in this light, decisions about their participation in committee affairs are among the most important that congressmen regularly make. From the member's point of view, involvement in committee work will affect the ability to achieve any of several political goals (Fenno 1973; Mayhew 1974); from the vantage of the institution, the aggregate patterns of participation will have several important implications. The range of members involved will directly affect the capacities of Congress as a representative body: who participates (in what ways, to what extent) will determine which values, interests, or geographic constituencies are represented at this crucial stage of the legislative process. Such patterns, in turn, will shape the policy decisions that emerge (or fail to emerge) from the committee rooms. And these patterns will have a direct bearing on how widely legislative authority is dispersed within an already decentralized institution.

Despite such implications, congressional scholars have paid little attention

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to the questions of who participates in committee decision making and why.[1] In this paper I take up these questions. My principal aim is to develop and test a model of committee participation.

The model begins with the premise that congressmen are purposive actors, but it relaxes the standard assumptions that goals are stable, ordered elements of the elite psyche. If, as Fenno (1973) says, not all representatives are alike in the goals they harbor, not all issues are alike in their relevance to particular goals. The behavioral effect of any particular goal, I argue, is contingent on the object of legislative action, subjectively perceived by the actor. Even for the same members and the same issues, not all decision-making forums will be equally suited to their purposes. I here attempt to identify the conditions under which specific goals will come into play and develop a measurement strategy consistent with this more complex motivational scheme.

If members are led to participate because of the interests that certain bills evoke, they also face a variable set of opportunity costs and resource constraints that structure their ability to act. Some start with greater advantages than others—in terms of political experience, substantive expertise, and lines of political communication; some have greater ability to pay the costs that participation entails. The theory of participation presented here, tailored to the context of committee decision making, specifies a set of factors that structure those costs as they vary member to member and bill to bill.

Committee Participation: Concept and Measurement

Unlike other, more studied forms of legislative behavior (the roll call vote, the committee assignment request), participation in committee decision making refers to no single, identifiable act. Like participation at the mass level (Verba and Nie 1972, chap. 3), it is a general category that subsumes a range of different political activities. Our first task is to consider how the concept is to be drawn. What are the possible modes of involvement at this stage of the legislative process?

I identify two here: *formal* and *informal* participation. Much of a committee's decision making activity takes place in formal committee and subcommittee markup sessions. Here the substantive issues are taken up for collective consideration; specific positions are debated; the legislation is summarized, amended, and voted on. Rules of parliamentary procedure are in force; hence all members enjoy the formal right to participate. Since the reforms of the early 1970s, moreover, the vast majority of markups have been held in open session so that formal participation has also been public participation. Such markups, in fact, tend to be well attended by committee outsiders: if not by the national press, certainly by representatives of the interests likely to be affected by the matter at hand.

At the same time, much of a committee's decision-making activity occurs informally—outside the context of an official markup. For instance, one of the most important stages occurs when members and their staffs decide the provisions of the original legislative vehicle. But members participate in other behind-the-scenes ways as well—in working out subsequent compromises, planning party strategy, building support for markup initiatives, and so forth.

Two reasons suggest that the distinction between formal and informal participation is an important one. First, the official markups are sometimes fast, pro forma affairs, revealing the roles played by only the two or three major players. Exclusive reliance on markup data thus increases the likelihood of measurement error. But the second and more important reason is theoretical, not methodological. Simply put, public and nonpublic participation may differ both in their causes and their effects. This possibility is suggested by the oft-cited distinction between "workhorses" and "show horses" in congressional policy making (Clapp 1963; Matthews 1959; Payne 1980). Workhorses are members who shun publicity and tend to their legislative duties; their goal is to gain the respect of their colleagues and thereby enhance their influence over particular legislation (Matthews 1959, 1067). Show horses, on the other hand, seek publicity and neglect serious legislative work. They use their formal access to the committee microphone to address some current or future constituency, and their activities are less likely to affect outcomes. In other words, while formal and informal participation should be related,[2] the former is more likely to tap a workhorse dimension of legislative behavior, the latter more likely to tap a show horse dimension. To fully comprehend the nature of committee participation, then, information regarding both modes is required.

Formal Participation

For most committees in both chambers, the available data on decision making in committee markups are surprisingly detailed and complete. The data for this exploration are drawn from the records of 17 bills marked up in the House Education and Labor Committee during the 97th Congress. They tap individual involvement in five separate activities at both the subcommittee and full committee stage: (1) attending markup, (2) voting on recorded roll calls, (3) offering amendments, (4) speaking during markup debate, and (5) setting the markup agenda (see Appendix).

Taken together, these data provide an almost exhaustive account of each member's formal participation, bill by bill. For the purposes of this exploration, however, a more economical measure of formal participation was required. The members were coded for each bill according to whether they engaged in each of the five activities, two of which were divided into more detailed subdivisions in order to retain more information.[3] The resulting seven dichotomous variables were used in a Guttman scale analysis and produced the following ranking:

0 = engaged in none of the activities
1 = attended
2 = voted
3 = spoke: minor participant in discussion
4 = spoke: major participant in discussion
5 = offered minor, technical, grammatical amendment
6 = offered substantive amendment
7 = played significant agenda-setting role

At the full committee level, the coefficient of reproducibility for this analysis was .97 and the coefficient of scalability .91, indicating a clearly hierarchical scale.[4] The analysis of subcommittee participation was likewise clear, producing a coefficient of reproducibility of .96 and scalability of .85. The Guttman scale scores, then, provide the summary measure of formal participation that I will use here (see Appendix).

Informal Participation

Like participation in the official committee markups, informal participation covers a wide range of specific activities. Unlike participation in markups, however, informal participation produces no written record of the member's involvement. Neither does it include only those activities in which a congressman participates in person. Prior to and outside the markups, much of the deliberation takes place at a staff level, with representatives participating through their agents. In order to assess the degree to which members participated in this informal, perhaps indirect fashion, tape-recorded interviews were conducted with the committee and subcommittee staff members who had primary responsibility for each of the bills in the sample. In the course of a semistructured interview about the particular bill's development, I sought to (1) identify the committee members who helped shape the original legislative vehicle and explore the role each played, (2) discuss the involvement of members in subsequent premarkup deliberations, and (3) identify the legislative assistants with whom the committee/subcommittee staff interacted during committee consideration of the bill.

Staffers' responses were then coded to reflect each member's behind-the-scenes involvement, using a five-point index ranging from no activity at one end to principal authorship at the other (see Appendix). For 15 of the 17 bills, at least two staffers were interviewed, one or more from the majority and one or more from the minority. For eight of the bills, both a majority committee staffer and a majority subcommittee staffer were interviewed. (The minority on Education and Labor does not have distinct subcommittee staffs.) Their responses, then, provide a general indicator of informal participation, one that proved highly reliable in two respects. First, the intercoder reliability was unusually high ($r = .94$).[5] Second, interstaffer reliability (for those bills where more than one staffer was interviewed) was high ($r = .86$), even when the comparison was confined to agreement between the majority and minority staffer ($r = .87$). In those cases where staffers disagreed, the responses were averaged.

Participation and Purpose

Consistent with other accounts of committee work in Congress (e.g., Carroll 1958; Clapp 1963; Fenno 1973, chap. 4), participation on House Education and Labor is highly selective. The measures of participation developed here bring this tendency into sharp relief. One senior Democrat, for instance, had a median markup score of zero: he attended neither a subcommittee nor a full committee markup for over half the bills in the sample. Yet on three other bills, his involvement was clearly major, offering and passing a total of sixteen substantive amendments and dominating the behind-the-scenes action. One freshman Republican, on the other hand, had a median markup of two (with a near perfect record of attendance and voting), yet he sat silently in the markup of almost every bill and did not offer a single amendment. In short, both the bill-to-bill and member-to-member variation is considerable.

How do we account for such patterns? Why do individual members participate (more or less) in the various questions that a committee considers? The explanation offered here focuses on two sets of factors: the representative's personal goals or interests and the structure of opportunities and constraints the member confronts in the decision making context.

Congressmen's Goals

Evidence both systematic and anecdotal tells us that contemporary House members have an almost limitless range of demands on their scarce time, energy, and legislative resources (e.g., Clapp 1963; Davidson 1981; O'Donnell 1978). Committee work is especially costly. Each bill on which a member participates directly diminishes time for other profitable activities: constituency relations, floor action, or involvement in some other committee bill, to name but a few. To participate the member must expect gains from the labor. I hypothesize here that congressmen selectively participate in order to advance certain political goals.

That congressmen are purposive actors, of course, is by now a common notion. Over the last decade-and-a-half, member goals have become central to theoretical work on legislative behavior (Ferejohn and Fiorina 1975; Sinclair 1983). Nonetheless, assumptions attending the concept's application in empirical research often remain implicit and unexamined (Eulau 1984, 611–15). Two assumptions will concern me here: first, that the motivational structure of individual congressmen is unidimensional; second, that the goals of congressmen are relatively stable across different legislative contexts.

The unidimensionality assumption comes in two variations. One genre of purposive models conceives of goals as general dispositions that are similar across individuals. At this level of abstraction, elites are motivated by self-interest or personal profit, which in legislative studies takes the more particular form of the desire for reelection. In Mayhew's now famous legislative psychology, congressmen are "single-minded seekers of reelection" (1974), and a literature now too lengthy to cite rests on this simple assumption.

Other studies, of course, render the mind of the congressman more complex. In the classic statement of this view, Fenno (1973) finds that congressmen are motivated by the goals of reelection, influence within Congress, and good public policy and suggests that "all congressmen probably hold all three goals" in different mixes (p. 1). But for the purposes of analysis, Fenno classifies members of a given committee according to the apparently "dominant" or "primary" goal in a relatively stable goal hierarchy (pp. 5, 7). This strategy, in turn, has led some scholars since Fenno to classify members and/or committees according to their central motivational tendencies (e.g., Hinckley 1975; Smith and Deering 1984). Most other studies that maintain some notion of mixed motives have tended to view them as being ordered, with reelection normally assumed to be the first concern of the representative, other goals becoming important when reelection is satisfied (e.g., Arnold 1979; Fiorina 1974). The effect of member goals on behavior is thus assumed to be relatively stable across different contexts, though their relative impact may change over the life-cycle of a representative's career (Fenno 1978; Fiorina 1974; Weisberg, Boyd, Goodman, and Gross 1982).

I here adopt a different view, one that derives from Kingdon's study of congressmen's voting decisions (1981, chap. 10). While the point is not explicitly drawn, Kingdon seems to suggest that the conventional assumptions cannot comprehend important variations in individual behavior. His "consensus mode of decision" not only incorporates Fenno's trilogy of goals, it comprehends the possibility that different goals may motivate a single congressman at different moments (i.e., on different votes) and that different goals may motivate different congressmen at a single moment (i.e., on a particular roll call vote). Under certain conditions, he finds, goals are "evoked" (see also March and Simon 1958, chap. 3).[6]

In the model of participation used here, then, the assumptions of unidimensionality and stability are relaxed. I assume that a representative may harbor any number of goals, more or less intensely. Their prominence in the member's motivational structure will vary depending on the issue being considered and the context in which he or she acts. The behavioral effect of any particular goal, in short, is contingent on the object of legislative action, subjectively perceived by the actor. The relevance of a bill to any one of a congressman's goals is likely to inspire greater activity, and the confluence of two or more goals should incite even greater participation. Whether individual congressmen are single-minded seekers of particular goals or whether a single goal can characterize a committee of individuals thus become empirical questions, not assumptions, in the purposive model specified here.

Measuring Motivational Intensities

If the assumptions employed here are correct, then, any attempt to measure goals-as-general-dispositions is theoretically second-best. Such dispositions exist as behavioral tendencies; they emerge in the course of individual action over time and across context. In committee studies, this point has been obscured by two common methodological problems. First, committee scholars have inquired about the goals affecting assignment seeking and simply assumed that such questions tap the motivations governing postassignment behavior. Second, they have had little data regarding motivational variations or intensities. As a result, the ability to estimate the effects of member goals on committee decision making has been severely limited.

The strategy required here, then, was to tap the relevance of each of several goals to each member's behavior on each of several committee bills. The practical problems in pursuing such a strategy, of course, are substantial. Access to representatives is often difficult to secure, and, in any case, many may be reluctant to talk analytically and frankly about their motivations. As a more practical if less direct approach, some scholars have interviewed congressmen's personal staff to obtain information about member goals. Bullock's oft-cited study of freshman motivations (1976), for instance, employs this approach, and his experience is encouraging. He concluded that "the material collected from staff is as accurate as that collected from members. This assumption was verified in those instances in which interviews were conducted with both the congressman and one or more of his staff" (p. 203). Smith and Deering (1984), likewise, relied partially on staff interviews in their recent multicommittee study.

For a number of reasons, the general strategy developed by Bullock was especially compelling here. In the first place, the need to obtain information regarding a series of issues required more time than a congressman was likely to give, even were access obtained. More important, the staff is itself intimately involved in the committee participation of the congressman, whom they keep briefed on committee activities. They look out for the member's interests and often act on his or her behalf. Information they provide is thus an important basis for the representatives' perceptions of bills, and their reactions are a basis for their perceptions. To use the language of Salisbury and Shepsle (1981a, 1981b), it is the subjective perceptions of the "congressman as enterprise" that the measures used here will tap. Several steps were taken to ensure the reliability of staffer responses, however.

First, I took special care to identify the staffer or staffers who would be best qualified to provide the required information. Second, I designed the interview schedule (discussed below) to exploit information that staffers were likely to have and willing to communicate; questions requiring staffers to read the motivational mind of their congressman were avoided. Whenever possible, finally, I interviewed two or more qualified staffers for each representative in order to systematically check the reliability of their responses, a strategy which proved possible for about 40% of the congressmen in the sample.

The interview schedule sought information regarding the relevance of each of the 17 bills to each of four possible goals:

Serving the District. As part of the interview, respondents were administered a two-page form asking them to rate the importance (major/moderate/minor/negligible) of each bill to their congressman's district. This proved to be a straightforward exercise. An important part of these staffers' jobs, in fact, is to keep their boss briefed on how committee legislation affects the constituency. The accuracy of staffer rankings, moreover, is supported by the agreement of responses when two or more staffers were interviewed for the same congressman. Over 96% of the rankings were within one of each other, and the interstaffer correlation was .80 (N = 238).

These ratings of district interest bill by bill, then, serve as the operational measure of each member's reelection goal (Fenno 1973; Mayhew 1974) or, more generally, of the desire to satisfy constituents' interests (Kingdon 1981, 246). The key assumption here is that this motivation will be evoked when issues arise that have an impact on the district. Specifically, we would expect members to participate more on those issues that they perceive to be highly salient to their constituents.

As I have conceptualized it here, however, the effect of district interest on a member's participation may cut two ways. If certain issues evoke intradistrict salience, they may also evoke intradistrict conflict (Fiorina 1974). Members may not only participate in order to promote their district's interests; they may avoid actions that could alienate specific groups. In the case of Education and Labor, such a scenario has historically held for the Republicans on labor issues (Fenno 1973). As one senior minority staffer noted: "We have a heck of a time getting new members to serve on the committee . . . and that's because of the labor side of things. We're always on the wrong side; the labor issues can only hurt you." In short, district concerns may lead Republican members to avoid visibility on labor issues as their districts' interests increase. To capture this effect, I use an interactive term which takes the value of the product of the district variable, the congressman's party (1 if Republican, 0 if Democrat), and a dummy variable for labor issues (1 if labor, 0 otherwise).[7] The expected effect, then, is negative.

Making Good Policy. Indicators for each congressman's goal of making good public policy (Fenno 1973) were measured in like fashion. Using a similar form, staffers were asked to rate how closely each bill related to their representative's general policy interests or commitments—interests that, from working with and observing a congressman week-in and week-out, staffers are in a good position to know. The important assumption implied in this operationalization, then, is that the desire to make good public policy is evoked when issues arise that are related to a congressman's particular set of policy interests or beliefs. Interstaffer reliability on these responses was .77.

Making a Personal Mark. The two goals discussed thus far are adapted from Fenno (1973). The third derives more directly from Lawrence Dodd's essay "Congress and the Quest for Power" (1977). Dodd argues that the most important goal of the contemporary congressman is to "make a mark" in a specific policy area (1977, 272–75).[8] In a decentralized Congress, the realization of this goal requires substantial effort in shaping legislation at the committee and subcommittee stage. As the notion of the workhorse suggests, moreover, members who seek to influence legislation must be especially active behind the committee scenes. The effect of this variable, then, should prove greater on informal than on formal participation.

The relevance of each bill to this motivation was coded (0–3) from responses to the question

> I've noticed that a congressman will sometimes select a specific issue or issue area where he wants to "make a mark"—where he might have

> some particular policy making influence. Are there any Education and Labor issues for which this would be true of Congressman ______? Do any of these bills fall into that area?

For each bill cited, probes were used to determine the extent to which the member sought to claim it as personal territory. Interstaffer reliability measured .91.[9]

Promoting the President's Agenda. Finally, I hypothesize that under certain conditions individual congressmen will participate in order to promote the president's agenda on matters within their committee's jurisdiction. In the parlance of Capitol Hill, a member "serves as point man" or "carries water" for the administration. This objective, of course, is no doubt related to the other goals already discussed. Ceteris paribus, members of the president's party have an electoral interest in seeing that his legislative program is successful, especially if they anticipate coattail effects in a future election. Those with policy interests similar to the president, moreover, should be likely to participate on matters high on the administration's agenda. But the staff interviews suggested that certain members take on the role of administration agents out of loyalty to party and president, far in excess of the direct electoral benefits they are likely to derive and independent of their own interests in the issues the administration is pushing.

The relevance of this goal was measured by an interactive term. Interviews with the relevant committee and subcommittee staffers were first used to determine which bills in the sample were high priority on the administration's agenda (coded 0/1). Personal staffers of Republican members were subsequently asked

> During 1981–82, the Reagan Administration was aggressive on a number of issues before Congress. Generally speaking, how committed would you say Congressman ______ was to carrying out the president's agenda in the area of Education and Labor?

Responses to this question were coded 0–3. The measure of each congressman's desire to promote the president's agenda is the product of these two variables. Interstaffer reliability measured .80.

Data on these five variables, then, provide reasonable if somewhat indirect indicators of individual members' motivations—motivations that may vary in intensity from congressman to congressman and bill to bill.

The Structure of Opportunities and Constraints

If member goals are important determinants of committee participation, they are not likely to provide a complete explanation. In allocating their scarce time and energy to committee affairs, representatives will not blindly pursue their interests when the benefits acquired are likely to be less than the costs incurred. I here explore factors that are likely to affect the opportunity costs and resource constraints the member confronts in deciding whether and to what extent to participate on a particular bill.

Subcommittee Membership

In the decision making process of the House committee, members of the subcommittee with jurisdiction over a bill enjoy a number of advantages, so much so that subcommittees are sometimes said to "govern" the legislative process (Davidson 1981; Dodd and Oppenheimer 1985). In the first place, the members are likely to have greater experience in the substantive issues their workgroups will address. Shepsle (1978) has shown that background in a policy area is itself one of the reasons for committee selection, and this reasoning undoubtedly holds for the subcommittee assignment process as well. In addition, many of the same issues recur in different bills within a subcommittee's jurisdiction so that the subcommittee member tends to have relevant legislative experience in both the politics and substance of a bill.

If subcommittee members start with superior expertise, they also enjoy greater resources to pay the marginal information costs (Hall and Evans 1985). Ceteris paribus, the subcommittee member will enjoy greater access to the professional staff assigned to a bill. The member's legislative assistants, likewise, are apt to have greater experience in the relevant issues. And both member and assistant are more likely to have well-developed lines of communication to interested actors both on and off the committee. The subcommittee member need not, as a result, give up as much in the way of beneficial activities elsewhere in order to pursue interests on a particular committee bill.

Subcommittee Position

Not all subcommittee members are equal, however. The opportunities for effective action are much greater for the chairman and ranking minority member of the reporting subcommittee. One of the key reforms of the early 1970s was to guarantee subcommittees their own staffs, and those staffs normally are hired and fired by the subcommittee chairman (Rohde 1974). Albeit to a lesser degree, the ranking minority member usually enjoys additional staff support as well, and both positions bring greater control over the subcommittee's agenda, the scheduling of meetings, and the progress of legislation. If such work is likely to conflict with other business the member finds important, the subcommittee leader is better able to adjust the nature and timing of events to suit his or her objectives. Thus chairmanship and ranking minority status should significantly enhance participation, both formal and informal.

If a subcommittee's chairmanship offers greater opportunities and resources, however, it also entails obligations and expectations. In particular, the chairman is obliged to manage reauthorizations of programs under the subcommittee's jurisdiction, major presidential initiatives, and other bills on which action is required. As several staffers noted, the chair is bound to be active even when not interested; this officer cannot participate as selectively as, say, the ranking minority member or full committee chair. Such role requirements suggest, then, that there is an interaction between chairmanship of the reporting subcommittee and the impact of the member's goals.[10] Such interactive effects should be negative.

Full Committee Position

The growth in subcommittee importance, as most observers have noted (e.g., Davidson 1981; Deering and Smith 1985), has diminished the importance of the full committee chairman and, one might surmise, of the ranking minority member. This is not to say, however, that the policy-making advantages of these positions have wholly evaporated. Chairmen and ranking members still retain authority over substantial staffs and budgets, as well as considerable influence over the timing and handling of bills referred to their committee. We should therefore expect that, ceteris paribus, full committee chairmanship or ranking minority status should significantly diminish the resource constraints felt by the individual member and enhance participation in committee decision making. If the claims about subcommittee government are accurate, however, the magnitude of such effects should prove smaller than those for the respective subcommittee positions.

Freshman Status

Until perhaps two decades ago, legislative scholars emphasized that the freshman representative is pressured by house norms to remain relatively passive in legislative affairs (e.g., Fenno 1966; Matthews 1960). Freshmen should be seen and not heard, more or less, and the failure to observe this admonition was likely to provoke sanctions by senior colleagues. More recent studies, however (e.g., Asher 1975; Ornstein 1981), have consistently argued that this norm has all but disappeared, suggesting that freshman status no longer depresses a member's role in committee decision making. The notion of participation costs sketched here suggests a different hypothesis. While group pressures may no longer restrain a new member's behavior, the freshman is more likely to be uninformed about the substance of a bill, naive about committee politics, and dependent on staff who are themselves inexperienced and inefficient. Overcoming such disadvantages requires a substantial investment of the member's scarce time and energy, an investment not likely to be sustained across a range of bills.

If the theoretical argument here is correct, then freshman status should depress participation in committee legislative work, other things being equal.[11] It is important to point out, however, that other things are not equal between freshmen and their more senior colleagues. In the first place, freshmen are less likely to have sought the committee, at least as a top choice. It is thus less likely that the various areas of the committee's jurisdiction will be well-suited to their goals. By incorporating specific indicators of member goals into the analysis here, however, I can control for this possibility. Second, freshmen may participate less because they are more likely to come from marginal districts; energy and attention are thus taken away from committee work in favor of other district-oriented activity—constituency relations, casework, more trips home, and the like (Fenno 1978, 215–24). I therefore control here for marginality, measured as a dummy variable that takes a value of one if the member won with less than 55% of the vote in the last election. By controlling for these other factors, the effect of freshman status should provide a reasonably good measure of apprenticeship in committee decision making.

Minority Status

The impact on committee participation of a member's party affiliation is likely to be complex. On the one hand, the minority member faces special barriers to participation, having less access to and affinity with individuals with formal power (i.e., committee and subcommittee chairmen), and enjoying fewer staff and other resources. In the drafting of the markup vehicle and other informal activities, these constraints should be especially serious. We should therefore expect minority status to depress one's informal participation significantly. But given that the minority is more likely to have been excluded from the informal deliberations, they should be all the more active at formal markup. The markup becomes their opportunity to react to and try to amend the majority vehicle. We should thus expect minority status to enhance formal participation.

A Multivariate Analysis: Results and Interpretations

According to the argument elaborated here, then, congressmen are led to participate by the goals that certain bills evoke. At the same time, they face a variable set of opportunities or constraints that structure their ability to act. In Table 1, I summarize the specific variables that comprise these two categories and present the regression estimates of their effects on four measures of committee participation. Columns 2 and 3 present the estimates for subcommittee and full committee markup participation, respectively, as measured by the Guttman scale scores described in the first section; column 4 presents the estimates for a combined measure of formal participation, which takes the higher of the subcommittee and full committee values;[12] column 5 presents the estimates for the index of informal participation, where the original scale (0–4) is transformed to the same scale as formal participation (0–7) to facilitate parameter comparisons.

Given the large number of random factors likely to affect participation on a given bill (scheduling conflicts, trips home, illness, etc.), the model performs fairly well, explaining 50% of the bill-by-bill variance in members' formal participation and 61% of the variance in their informal participation. It is especially encouraging, too, that (1) the estimates for formal and informal participation are remarkably similar even though these behavioral modes were measured using two different measurement strategies and qualitatively different data sources; (2) the differences that do appear, as I will discuss shortly, are consistent with the

Table 1. Multivariate Analysis of Participation in Committee Decision Making: House Education and Labor, 97th Congress

	Unstandardized Regression Coefficients			
Independent Variables	Subcommittee Markup	Full Committee Markup	Formal Participation	Informal Participation
Goals				
Serving the district	.65[a]	.25[a]	.30[a]	.15[c]
Republican-labor interaction	-1.15[c]	- .24	- .27	- .22
Making good policy	1.11[a]	.50[a]	.54[a]	.36[a]
Making a personal mark	.39	.61[a]	.59[a]	1.05[a]
Promoting the president's agenda	.22	.60[a]	.49[a]	.22
Opportunities/constraints				
Membership, reporting subcommittee	—	.41[a]	.80[a]	.85[a]
Chairmanship, reporting subcommittee	6.97[a]	5.89[a]	5.56[a]	6.54[a]
Ranking minority status, reporting subcommittee	2.64[a]	1.87[a]	2.11[a]	1.57[a]
Chairmanship, full committee	—	1.65[a]	1.69[a]	1.42[a]
Ranking minority status, full committee	—	1.47[a]	1.27[b]	.18
Freshman status	-1.38[c]	- .84[a]	- .95[a]	- .79[a]
Marginal district	.61[c]	.10	.27[c]	.15
Minority party	- .11	.41[b]	.42[b]	.26
Chair-goal interactions				
Subcommittee chair, district service	- .55	- .34	- .33	- .58[c]
Subcommittee chair, good policy	-1.22	- .55	- .58	- .51
Subcommittee chair, personal mark	- .35	- .41	- .41	- .45
Number of cases	96	493	493	493
R^2	.72	.48	.50	.61

[a] Significant at .01 level.

[b] Significant at .05 level.

[c] Significant at .10 level.

theoretical differences in the nature of these two modes; and (3) the results are reasonably similar when one moves from subcommittee to full committee even though the subcommittee stage entails a nonrandom subset of the bills subsequently marked up at full committee, includes a different population of actors, and (given that several variables at the full committee level drop out at the subcommittee level) requires a slightly different explanatory model. Of greater theoretical interest here, however, is that with few exceptions the hypothesized relationships discussed above are clearly borne out.

The Motivational Complexity of Congressmen

The theoretically most important finding of this analysis is the support given to the model's complex picture of congressmen's motivations. The most basic (if least remarkable) conclusion to be drawn is that congressmen are much more than single-minded seekers of reelection. The parameter estimates for all four goals are statistically significant in the model of formal participation, for instance, and the constituency motivation has the smallest impact of the four. In one respect, of course, this is not surprising. Education and Labor is normally thought of as a policy more than a reelection committee, and that general tendency finds support here. In participation both informal and formal, at subcommittee and at full committee, the policy coefficient is consistently correct in sign, statistically significant, and larger than the coefficient for the district service variable.

This is not to say, however, that constituency concerns are never on the minds of representatives as they decide whether and to what extent they will participate in Education and Labor decisions. The coefficients for district service are consistently positive and significant; the coefficients for the Republican-labor interaction, moreover, are consistently negative, suggesting that under certain conditions individuals may avoid issues that cause them political problems at home. At one level, then, the analysis supports the findings of scholars (e.g., Perkins 1980; Smith and Deering 1984) who suggest that certain committees may be characterized as having "mixed goals." But the more important implication is that such studies understate committee members' motivational complexity. While good policy and reelection may now stand out as the principal reasons for seeking committee assignments (Smith and Deering 1984, 90), these are only two (and perhaps not even the most important two) goals that shape the behavior of congressmen in postassignment decision making. In both formal and informal participation, one of the strongest motivational influences is the member's desire to make a mark.[13] Likewise, the member's interest in promoting the president's agenda is consistently correct in sign and proves significant in the model of formal participation.

The results of Table 1 likewise show that specific goals are evoked by the legislative context in which the member acts. As I hypothesized, the district motivation is a significant determinant of a member's public participation, but the coefficient is half as large for participation behind the scenes and borders on statistical insignificance. At the same time, the effect of the member's desire to make a mark is twice as large for behind-the-scenes as it is for public participation. To the extent members are motivated by reelection, they are apt to play the role of show horse, to exploit the public nature of the markup and, as Mayhew (1974) argues, to pursue electoral objectives without emphasizing substantive legislative work. But the individual interested in making a mark must be something of a workhorse. Significant influence over the substance of a bill seldom comes without involvement in such activities as writing, negotiating, and building support for its major provisions. In sum, formal participation tends to be more constituency-oriented than informal participation; informal participation tends to be more influence-oriented than formal participation—precisely the findings the workhorse/show horse distinction led us to expect.

The presidential loyalty motivation likewise exhibits important but predictable differences across the two participation modes. While this goal appears to be an important determinant of public participation, no such claim can be made for participation behind the scenes. During the 97th Congress, the Reagan administration initiated a number of attacks on programs dear to the Democratic majority of Education and Labor, and the Democrats responded with ideological solidarity. On the administration's high agenda items, in particular, administration loyalists were afforded few opportunities to promote the president's agenda prior to the official markup.

The presidential loyalty motivation, then, is contingent on the behavioral context in which the member acts—a context here characterized by ideological division, with the administration pitted against the committee majority. If this argument is correct, however, we should not expect the effects to be constant over time. Because the sample here covers two years, one (1981) spanning Reagan's honeymoon period and another (1982) characterized by greater congressional independence (especially in the House), we can examine the extent to which loyalty to the administration wanes as the president moves into his second year. A reestimation of the model appears in Table 2 in which I separate the presidential loyalty variable into first- and second-year effects.

The results are interesting in several respects. First, the presidential loyalty motivation is in fact a strong, positive determinant of formal participation during Reagan's first year in office, only to prove statistically insignificant during the second year. Equally interesting, however, is that the pattern proves just the reverse for informal participation. In the context of the highly partisan, executive-led politics of 1981, Republican members apparently found active, visible support for Reagan not only politically more attractive, they found the majority less willing to negotiate with them behind the scenes. By 1982, however, with Reagan declining in popularity and his budget cuts boding ill for Republicans in the midterm elections, the presidential loyalty motivation had generally diminished, and the remaining loyalists had gone behind the scenes. In sum, presidential loyalty influences informal participation, but its effect is indeed contingent on other political conditions.

In sum, the variations across participation modes and legislative sessions support the argument that member goals are less stable than has normally been assumed. But the contention here has been more specific: I have suggested that an individual member may be led to participate by different objectives, depending on the member's perception of the issue at hand. On this point, the evidence of the foregoing analysis is only indirect. The pattern of the parameter estimates shown could be produced by differently motivated persons, single-mindedly pursuing individual goals.[14] The full data set was thus broken into congressman-by-congressman subsets, and the model was estimated for each one. While their N is small (17), such analyses provide some insight into the individual-level motivations. Two strategies were used here. The first was to test whether the signs of the goal coefficients were consistently positive. If the motivational psychology of congressmen is characterized by single-mindedness, we should expect that once the largest positive coefficient for each congressman is eliminated, the coefficients of the remaining goal variables should randomly be distributed around zero. This did not prove to be the case, however. Only 23% of such coefficients were negative, with the mean of all coefficients positive (.21). The second was to examine the relative size of the goal coefficients for each representative. In 19 of the 29 cases (66%), the second largest positive coefficient was at least half the size of the largest and greater in magnitude than its counterpart in the analysis of the full data set.

A final angle on the individual's motivational complexity derives from the regression analyses, conducted bill by bill. If members have stable goal structures that shape their behavior regardless of the decision context, we should expect

Table 2. Multivariate Analysis of Participation in Committee Decision Making, Including "Period Effects": House Education and Labor, 97th Congress

	Unstandardized Regression Coefficients			
Independent Variables	Subcommittee Markup	Full Committee Markup	Formal Participation	Informal Participation
Goals				
Serving the district	.65[a]	.25[a]	.26[a]	.12
Republican-labor interaction	-1.15[c]	- .21	- .27	- .18
Making good policy	1.10[a]	.49[a]	.54[a]	.39[a]
Making a personal mark	.43	.60[a]	.59[a]	1.03[a]
Promoting the president's agenda, 1st year	.19	.71[a]	.68[a]	- .13
Promoting the president's agenda, 2d year	—	.36	.32	.41[c]
Opportunities/constraints				
Membership, reporting subcommittee	—	.44[a]	.84[a]	.83[a]
Chairmanship, reporting subcommittee	6.99[a]	5.82[a]	5.52[a]	6.47[a]
Ranking minority status, reporting subcommittee	2.62[a]	1.85[a]	2.11[a]	1.57[a]
Chairmanship, full committee	—	1.65[a]	1.69[a]	1.35[a]
Ranking minority status, full committee	—	1.50[a]	1.31[a]	- .08
Freshman status, 1st year	-1.46	- .79[b]	- .78[b]	- .85[a]
Freshman status, 2d year	- .62	- .47[a]	- .54[a]	- .50[a]
Sophomore status	.21	- .12	.07	- .43[a]
Marginal district	.59[c]	.11	.17[c]	.18[c]
Minority party	- .15	.43[b]	.42[b]	.34[c]
Chair-goal interactions				
Subcommittee chair, district service	- .56	- .33	- .33	- .55
Subcommittee chair, good policy	-1.22	- .55	- .58	- .54
Subcommittee chair, personal mark	- .39	- .41	- .41	- .44
Number of cases	96	493	493	493
R^2	.72	.47	.50	.61

[a] Significant at .01 level.
[b] Significant at .05 level.
[c] Significant at .10 level.

relative stability in the coefficients for the goal variables across the 17 bills. The variation that appears, however, is considerable. Coefficients on the district motivation ranged from around 2.0 on the 1981 Budget Reconciliation recommendations to slightly negative on a bill that would provide a one-time $50,000 death benefit to survivors of federal police officers. Coefficients on the policy goal ranged from near zero on a bill to reauthorize a minor educational program to 1.45 for the Job Training Partnership Act—the committee's most important initiative of the session. Two bills, in fact, evoked none of the goals, with the several leadership positions accounting for most of the variance explained. In sum, a range of evidence suggests that the approach to congressmen's motivations used here is warranted.

Institutional Position and Resource Constraints

If the multivariate analyses suggest that members are purposive, they provide clear evidence that certain opportunities and constraints structure their behavior as well. The most striking results, in fact, are those associated with membership in the subcommittee of jurisdiction. All three of the relevant coefficients are correct in sign and clearly significant, regardless of the measure or mode of participation analyzed. As hypothesized, subcommittee membership by itself enhances participation even if we focus only on the full committee markup. Especially dramatic are the effects of the subcommittee leadership positions. As one would expect, the impact of chairmanship is greater than that of ranking minority status, but both are exceedingly powerful determinants of behavior.[15] The importance of subcommittee membership and position are even more apparent in legislative action behind the scenes, where professional staff and well-developed lines of communication are crucial.

At the same time, we should not interpret the leadership coefficients solely as evidence of resources exploited. They also tap the fact that formal position can carry official obligations, leading the ranking member to serve as spokesman for his or her party's position. As I argue above, this is especially true for the subcommittee chair. This argument finds consistent support in the multivariate results. While none of the interactions between subcommittee chair and member goals is statistically significant, they are uniformly negative across the several models. Participation, such patterns illustrate, is less purposive for those with well-defined role requirements.[16]

The multivariate results likewise show that full committee leadership position enhances a congressman's participation in decision making activity. Superior legislative background, staff resources, and agenda control generate opportunities to intervene in a wide range of committee decisions. It is worth emphasizing, however, that such effects are apt to vary considerably from committee to committee, leadership style being a somewhat idiosyncratic thing. The findings here accord with the reported styles of both Chairman Carl Perkins and ranking Republican John Erlenborn. While staff members consistently emphasized the importance of Perkins's role in committee deliberations, he gave considerable latitude to his subcommittee chairmen and normally deferred to their judgment (Fenno 1973, 287–88; Smith and Deering 1984, 179). Of special interest, then, is not that full committee chairmanship is statistically significant but that it is not greater than it is ($b = 1.69/1.42$), especially when compared with the parameter estimates for both the subcommittee chair ($b = 5.56/6.54$) and subcommittee ranking minority member ($b = 2.11/1.57$). Similar patterns obtain for Erlenborn, who, according to senior committee staffers, followed his predecessor's practice of letting the subcommittees go their own way.[17]

The Status of the Freshman, Revisited

Perhaps the most interesting results of Table 1 are those associated with freshman status. The negative effect of this variable is consistent and striking across each of the four models, even equalling the impact of subcommittee membership. However much their status may have improved, freshmen apparently have not achieved participatory equality, even when one controls for such things as marginality, full and subcommittee leadership position, and the relevance of committee legislation to member goals. This is not to say, however, that House norms enforced by senior colleagues keep newcomers off the committee stage, even after they are ready and willing to participate. Fenno (1973, 101–5) concluded that such norms were already moribund on House Education and Labor in the late 1960s, and his finding was confirmed by the staffers interviewed here. Rather, apprenticeship of a different sort operates in committee decision making, one deriving from the relative costs and resource constraints faced by the newcomer in learning a complex political process and sometimes esoteric subject matter.

Additional support for this argument is provided by Table 2. If the concept of apprenticeship used here is correct, we should expect to see the negative effects diminish as the members and their staffs gain experience and expertise over the course of the first term. Freshman status is here separated into its first- and second-year effects, and a dummy variable for sophomore status is included to provide a point of comparison. The patterns of coefficients are the same in the estimation of all four models. The negative effect is strongest in the first year, smaller but still significant in the second year, and smaller still during the sophomore term, remaining significant only for behind-the-scenes participation. By the time of the second term, we might surmise, members have become more familiar with the decision making process, but they and their staffs have not fully developed informal lines of communication with the relevant senior actors.

Minority Status and Institutional Leverage

As hypothesized, minority status enhances participation in formal deliberations but not in informal ones; however, the evidence on the former is somewhat mixed. The effect on markup participation proves positive and significant in the full committee but not the subcommittee model. One reason for this may be that majority preferences are in relative flux at the subcommittee stage but become solidified in the form of the full committee markup vehicle. The full committee markup thus provides the final and most visible opportunity for the minority to express its objections as the bill moves to the floor.

That the coefficient for minority status is positive in the model of informal participation is also somewhat surprising, though the staff interviews pointed to elements of the institutional context that render this finding less anomalous. For the first time in nearly three decades, the 97th Congress ushered in Republican control of both the Senate and the White House. While this immediately and dramatically enhanced the status of Senate Republicans, Education and Labor staffers also cited positive effects for Republicans in the House. "Because we have a Republican Senate," one minority staffer explained, the committee majority "has come to recognize the value of getting the minority on board. . . . If they tick off the Republicans totally, what we will usually then do is get to the Senate people and say 'We need to kill this bill.' That unspoken threat—the realization that the Republicans control the Senate and the White House—has been very important. During the Carter Administration you found that [the committee Democrats] were much less willing to accommodate Republican concerns." The institutional leverage of the Republican party, in short, created greater opportunities for minority participation generally (see also Fenno 1973, 75) even though it diminished the willingness of Democrats to negotiate with conservative Republicans acting as administration loyalists.

Conclusion

The model of committee participation developed here begins from the premise that congressmen are purposive actors, who participate because they want to. What members want is to advance their political goals. But having borrowed on a now common theme in the literature on legislative behavior, I here develop a more complex motivational psychology. If, as Fenno (1973) says, not all representatives are alike in the goals they harbor, then not all issues are relevant to particular goals. For the same member of Congress and for the same issue, not all legislative contexts will be equally suited to the member's goals.

This view of purposive behavior finds strong support in the results presented here. Four different goals—serving district interests, making good policy, making a political mark, and promoting the president's agenda—affect behavior on House Education and Labor. Subsidiary analysis suggests, moreover, that different goals are evoked by different issues and that motivational effects vary in predictable ways across legislative contexts. Not only are congressmen not, in Mayhew's phrase (1974), "single-minded seekers of reelection," to characterize them as single-minded seekers of any goal does considerable injustice to the complex set of motives that shape their behavior, even on a single committee and a small sample of bills.

According to the model developed here, however, congressmen cannot always do what they want. In deciding whether and to what extent to participate in particular decisions, the committee members face a variable set of opportunities and constraints that structure their ability to act. Members and, especially, leaders of the subcommittee of jurisdiction enjoy advantages in terms of information, staff, and lines of political communication—resources that subsidize the considerable opportunity costs that participation entails. So do the chairman and ranking member of the full committee despite the relative diminution of these positions in the last decade. Each of these positions exhibits a strong, positive effect on participation in committee decision making even when we control for member interests. At the same time, the freshman member faces analogous resource constraints. In participation both formal and informal, at full committee and subcommittee, freshman status significantly diminishes a member's decision making role. The implication here is that apprenticeship remains important for understanding legislative behavior, though the sociological assumptions commonly imbedded in the concept no longer hold.

Perhaps the most important implications of this study, however, lie less in what it does than in the directions it suggests. For the study of legislative decentralization, the findings regarding subcommittee members and leaders pose several questions worth careful analysis (see also Hall and Evans 1985; Sinclair 1983). Under what conditions do subcommittee nonmembers actively review subcommittee actions? To what extent do the relative advantages of subcommittee position translate into influence over committee outcomes? To what extent do such advantages structure activity and influence on the chamber floor? If we are to develop a theory of influence appropriate to a decentralized Congress, patterns of selective participation at each stage of the legislative process require greater attention (Hall 1986, chaps. 6 and 7).

For theoretical work on committee decision making, likewise, the present study suggests a new line of inquiry. Owing to Fenno (1973), one tradition in this area emphasizes member goals as the principal determinants of committee decisions. Price (1977; 1985) persuasively argues, however, that goals have proven too general to explain substantive policy outcomes. More formal models, in turn, build upon individual preferences to explain decisions, but such work has shown too little concern with observed patterns of committee behavior (Panning, 1983; but see Gilligan and Krehbiel 1986; Shepsle and Weingast 1986). The study of participation here suggests a potentially fruitful connection between these two traditions: the general interests that empirical scholars refer to as "member goals" shape the decision to participate; it is the preferences of participants that structure the legislative outcome.[18] An education and labor bill to reform public pension regulations, for instance, was almost a simple summary of the preferences of subcommittee chairman Phillip Burton and ranking Republican John Erlenborn. The interests of very few others were even modestly evoked; their (ill-formulated) preferences thus never entered the decision-making calculus. A theory of committee decision making, in short, should not only tell us how players will bargain but how it happens that members come to be players in the first place.[19]

For students of representation, finally, committee participation should prove an important concern. Committees and subcommittees, we know, are not microcosms of the parent chamber. The way in which committee assignments are made guarantees that these groups are deep in "interesteds" and unrepresentative of the regional, ideological, and seniority patterns evident in the parent chamber (Shepsle 1978, 259). But, as I suggest in this paper, the membership rolls provide only an inexact sketch of who makes the policy decisions at the committee level. Participation is selective and purposive. The range of interests, values, and geographic constituencies thus expressed on any given issue is likely to be even more narrow, the bias of interests represented more severe, than the study of committee assignments can reveal. Such behavioral patterns require greater attention if we are fully to understand the phenomenon of representation in a decentralized Congress.

Appendix

Sampling

The samples of members and bills analyzed here are very close to being the full populations for Education and Labor during the 97th Congress. I attempted to gather the appropriate data from both staff interviews and committee records for all 33 of the congressmen who sat on the committee for the entire congress, and I succeeded for 29 (88%). The sample of bills (17) represents 61% of all bills that received any kind of markup action during the two-year period. In choosing the sample, I included bills from the various subcommittees in rough proportion to the number that they reported for full committee action, but I generally excluded bills for which the legislative markup (both at full and subcommittee) was merely perfunctory. In the end, the sample reflected a mix of major and minor bills and spanned a range of domestic policy areas: education, welfare, labor-management relations, child abuse, national employment policy. The records were missing for the subcommittee markups of two bills in the sample.

Formal Participation: Data and Coding

Data on members' markup participation were gathered separately for subcommittee and full committee and coded by the author. The following criteria were used:

Attendance. A member who appeared at one or more of the markup sessions held on a given bill was credited with attending.

Voting. A member who personally responded to one or more roll calls during a markup was coded as a voting participant. While proxy voting is commonly practiced in House committees, senior committee staffers noted that specific voting instructions are seldom provided by the member relinquishing a proxy. Thus, voting by proxy was coded here as not having voted.

Speaking. Participation in markup debate was measured by counting the number of transcript lines each member spoke on a given bill and then classifying the counts into major and minor speaking categories. In making the classifications, several thresholds were tried without producing changes in the codes for more than a very few observations. I thus settled on the one that seemed intuitively most reasonable: if a member spoke more than "his or her share" (1 / 33 members, or 3%) of the lines in the markup transcript, the member was classified as a major speaker. If the percentage of lines spoken was greater than 0 but less than 3%, a member was classified as a minor speaker. In counting the lines of a given meeting, administrative remarks by the presiding member were eliminated, as were responses to roll call votes and quorum calls.

Amending. The data on this activity were coded according to whether each amendment offered (1) would make technical or grammatical changes in the legislation or was otherwise cited as being minor and noncontroversial or (2) entailed substantive changes in the legislation. Where there was doubt about the appropriate category, the amendment was assumed to be substantive.

Agenda setting. A member was credited with setting the markup agenda if that member (1) called up or was otherwise responsible for authorizing the bill used as the markup vehicle or (2) offered and succeeded in passing an amendment in the nature of a substitute to the vehicle. While the markup records normally revealed who the principals were in constructing the vehicle, in a few cases committee staffers identified other members who played a significant agenda-setting role. In those cases, the member was coded on the basis of the staff reports.

As noted in the text, Guttman scale analysis was used to reduce these several kinds of data into a summary measure of formal participation. Using this technique, of course, generates the loss of a significant amount of information. A member's amending activity, for instance, is reduced from the number of amendments offered at a particular markup to whether the member offered any at all. But, with the exception of speaking and amending, the activities tended to be either dichotomous or trichotomous so that more sophisticated scaling was not possible. Thus it was not obvious what method of data reduction would be best. I here opted for one that captured several different kinds of activity, could be replicated easily, and provided comparability across bills in different committees and subcommittees.

Informal Participation: Data and Coding

The data on informal participation were gathered through committee staff interviews conducted in late 1983 and early 1984. The individuals with primary responsibility for staffing each bill in the sample were identified by the minority and majority staff directors. Of the 23 that were identified (several staffers were responsible for more than one bill), 22 agreed to an interview, producing a response rate of 96%. Staffers were guaranteed anonymity, and when permission was given (91% of the time), the interviews were recorded. The transcripts were then coded independently by the author and a research assistant, using the following categories:

0 = none. No mention of congressman or congressman's agent in any behind-the-scenes capacity.

1 = minor. Member/agent cited as expressing support/opposition for or interest in some aspect of the bill, but reference to behind-the-scenes activity occurs rarely and only in passing.

2 = moderate. Member/agent mentioned as actively pushing one specific point of view or provision or as being party to a key compromise but not a frequent participant in behind-the-scenes deliberations.

3 = major. Member/agent one of the central players in most informal negotiations, compromises, staff meetings, and so forth. Perhaps some role in drafting provisions of markup vehicle but not a principal author.

4 = a principal author. Member/agent clearly responsible for the shape of the original vehicle, as well as playing leadership role in all subsequent negotiations, compromises, staff meetings, and so forth.

As noted in the text, intercoder reliability was high. Where disagreements did occur, we resolved them through a joint reconsideration of the item. When a mutually agreeable solution was not possible, the codes were averaged.

Personal Staff Interviews

Data on each member's goals were gathered from interviews with the staffer(s) who worked most closely with the member on Education and Labor affairs. These individuals were identified for me by senior committee staffers. The "best person to talk to" was interviewed for 29 of the 33 members. For 12 members (41% of the sample), a second qualified staffer was interviewed in order to check the reliability of the responses (see discussion in text). Anonymity was guaranteed, though these interviews were not recorded. Notes were taken during the interview and expanded immediately afterwards. The interviews were conducted from March to May 1984.

Notes

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1. The most important exceptions are Price (1972) and Fenno (1973, chap. 4). Other works that touch on the subject include Carroll (1958, 27–37), Clapp (1963, 264–68), Manley (1970, 90–97), Payne (1980), Perkins (1980), and Smith and Deering (1984, chap. 5). The importance of participation in political decision making has been a more direct concern of students of organizational behavior, however (see, for instance, Cohen, March, and Olson 1972; and March and Simon 1958).

2. Even if key provisions or compromises are proposed and discussed prior to markup, they must then be offered in markup (either as part of the markup vehicle or as separate amendments to it), justified or opposed by the actors involved, and then formally ratified or rejected. Thus, even though much may go on outside the markup, the markup

offers some indication of who participates behind the scenes.

3. It is important to emphasize that the unit of analysis here and throughout is neither the congressman nor the bill. Rather, it is the act of participation or, to be more precise, the series of concrete acts that can be summarized as the participation of congressman *i* on bill *j*. The full committee data set thus includes the number of congressmen (29) times the number of bills (17) in the sample, or 493. See also Kingdon (1981, 15–16).

4. There are, however, certain inherent reasons why the data should be hierarchically structured. For instance, it is logically impossible for one to speak at a committee markup without attending it. The more detailed set of categories, in fact, builds greater hierarchy into the scale. As a more reasonable test, a separate analysis was conducted that included only those items not logically ordered. The results of this analysis were likewise clear: the coefficient of reproducibility was still .95 and the coefficient of scalability, .79.

5. Lest the coder reliability appear better than it is, I should point out that for all but two bills in the sample (indeed for most bills that move through the committee system), only a fraction of the members was at all involved in behind-the-scenes activities. The majority of members are thus never mentioned, their participation score obvious. When the test is confined to the subset of cases where a congressman is mentioned in the interview, however, the reliability remains satisfactory: $r = .78$.

6. Some notion of a goal hierarchy remains in Kingdon's model in that congressmen first consider the relevance of a vote to their districts; only after this will they consider the goals of good polcy and intra-Washington influence. But Kingdon suggests that specific goals are evoked by the issue under consideration so that certain goals may simply drop out of the decision calculus (1981, 247). While his analysis focuses primarily on the goal of reelection, Shepsle's study of committee assignment requests (1978, esp. pp. 45, 103–4) entails similar motivational assumptions.

7. These indicators thus indirectly tap two dimensions of policy content—salience and conflict—that other legislative scholars have cited as important determinants of legislative behavior (e.g., Fiorina 1974; Hinckley 1975; Kingdon 1981; Price 1978). The difference here is that I capture the subjective perception of salience and conflict, specifically focused on the member's own constituency. I thus need not assume them to be objective characteristics of a bill nor that, say, national salience, intersubjectively perceived, is equivalent to political salience for the individual congressman. Regarding such assumptions, see Hinckley 1975, 545–56; and Price 1978, 568–69.

8. The area the member "carves out," of course, is often one where he or she has well-defined, intense policy interests. The interviews suggested that this does not always hold, however, and the correlation between the indicators for the policy and personal mark goals was relatively modest ($r = .27$).

9. The high reliability in this case derives from the fact that any given congressman can stake credible claims to only a very few issues. Thus most of the bills were never cited in the course of an interview and were coded zero. Still, staffers were fairly consistent in identifying bills for which the congressman felt some territorial interest, and the accompanying discussions confirmed that they interpreted the question properly. Where the test of reliability is confined to those bills where there was at least one nonzero value, the interstaffer correlation was .68.

10. For the period of this study (1981–82), the presidency and the committee were controlled by different parties, so that the goal of promoting the president's agenda is zero for all observations of subcommittee chairmen. It thus drops out of the analysis here.

11. To a lesser degree, the same argument applies to new transfers. None appeared in the sample under study here, however, so that transfer status drops out of the model.

12. This summary measure is useful because the subcommittee markup measure does not comprehend participation at the full committee stage, and the full committee measure does not comprehend the fact that subcommittee members need not (always) repeat at full committee what they have achieved in subcommittee.

13. The results are somewhat mixed on this point however. Given the growing importance of subcommittees in writing legislation (Davidson 1981), we should expect the effect of this variable to be greater on subcommittee participation than full committee participation. While the coefficient is correct in sign at the subcommittee level, however, it is not significantly far from zero. One problem this no doubt suggests is that I am not altogether able to disentangle the effects of the congressman's desire to make a mark (as it is evoked by a given bill) from the effect of subcommittee leadership position.

14. Were this the case, however, we would expect to see significant goal-congressman interaction effects. The equation was thus reestimated with dummy variables for each congressman interacting with each of the goal variables. In the estimates for formal participation, only nine out of a possible 92 interactions were significant at the .10 level, the variance explained was not appreciably enlarged, and the other coefficients were relatively stable. Nearly identical results obtained for the reestimation of informal participation as well.

15. This is not to say, however, that the ranking members or the subcommittee units control committee outcomes. Making inferences about legislative influence from such data requires considerable caution (see Hall and Evans 1985; and Hall 1986, chap. 6).

16. Insofar as I am able to measure, at least. Sub-

committee chairs tend to score at or near the top of the scales used here; thus the negative coefficients may partly be due to the scales being bounded. Considerable interview evidence corroborated the interpretation offered here, however (see Hall 1986, chap. 3).

17. Erlenborn was also new to the position. In fact, he did not assume ranking status until the death of John Ashbrook midway through the period studied here.

18. Of course, the distinction between goals and preferences is hardly a clean one; in fact, the two terms are often used interchangeably. As I use them, the evocation of some general goal determines whether (and with what intensity) a member will have crystallized preferences and the dimensions along which those preferencs will be ordered (see Hall 1986, chap. 7).

19. The tendency of formal theorists to ignore nonparticipation follows from the exclusive attention given to one form of participation—voting. Since votes are more or less valuable commodities and the act of voting requires little in the way of time and resources, nonparticipation is seldom a rational option. However, the more general assumption that participation is cost free, this study suggests, is wholly unrealistic.

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INSTITUTIONAL STRUCTURE AND THE LOGIC OF ONGOING COLLECTIVE ACTION

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Work by Axelrod, Hardin, and Taylor indicates that problems of repeated *collective action may lessen if people use decentralized strategies of reciprocity to induce mutual cooperation. Hobbes's centralized solution may thus be overrated. We investigate these issues by representing ongoing collective action as an* n-*person repeated prisoner's dilemma. The results show that decentralized conditional cooperation can ease iterated collective action dilemmas—if all players* perfectly *monitor the relation between individual choices and group payoffs. Once monitoring uncertainty is introduced, such strategies degrade rapidly in value, and centrally administered selective incentives become relatively more valuable. Most importantly, we build on a suggestion of Herbert Simon by showing that a hierarchical structure, with reciprocity used in subunits and selective incentives centrally administered, combines the advantages of the decentralized and centralized solutions. This hierarchical form is more stable than the decentralized structure and often secures more cooperation than the centralized structure. Generally, the model shows that the logic of repeated decision making has significant implications for the institutional forms of collective action.*

The logic of collective action is rather discouraging. As Mancur Olson stressed in his pioneering work (1965), common interests may not sustain common effort. Although every potential member of an interest group may benefit from a piece of legislation, if a person could enjoy the fruits of the legislation without contributing to the collective cause, then pure self-interest will induce that person to free ride on the actions of others. And as for interest groups, so for political parties, labor unions, professional associations, indeed any collectivity that relies on voluntary action. The logic cuts a wide swath across political organizations.

Not all organizations, however. Olson argued that small groups tend to be privileged in that they are more likely to realize collective goals effectively. It is large groups that suffer most acutely from free rider problems, to the point that a large potential group may fail to form at all. Olson and others who have examined the tendency of large groups to remain latent have proposed four reinforcing reasons:

1. If the benefit in question involves rival consumption—Hardin's commons, for example—then the per capita share of benefits falls as the collectivity increases.

2. On the production side, the contributions of individuals may become less important to overall group performance as size increases.

3. Problems of communicating, organizing, deciding—the transaction costs of

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interest groups—intensify in large groups.

4. Social incentives, positive ones such as feelings of solidarity and negative ones such as shame, are weak and unreliable in large organizations.

Olson, recognizing that some large groups (labor unions, professional associations, farmers' organizations) have been viable and often effective for prolonged periods, argues that this is due to a finessing of collective action problems. In such groups it is selective incentives that stabilize participation; the collective goals are achieved as a byproduct of individual contributions aimed at the selective rewards (1965, 132). Where selective incentives are infeasible—for example, cartels, which are legally precluded from punishing firms that break the cartel's policy—cooperation is fragile, prone to dissolving into bouts of mutual defection.

Olson's work stimulated a fundamental rethinking of the origin, strategy, and performance of interest groups. Inevitably, this ambitious and influential theory has been criticized on several fronts. It has been noted that his prediction about the effects of size on overall group performance does not apply to nonrival goods for which the significance of individual contributions declines slowly as a group increases in size (Chamberlain 1974). In such circumstances, total group output rises with total membership. Thus, collective action problems do not inevitably become prisoner's dilemmas as groups become larger (Taylor 1976, 17–25).

Most importantly, perhaps, Olson's analysis is static, depicting collective action as a one-shot game. When a single-period game satisfies the conditions defining a prisoner's dilemma, then the collective action problem is indeed serious, for it is well known that in the one-shot prisoner's dilemma, defection—free riding—is a dominant strategy. This is an extremely strong property. When one strategy dominates all others, the dominant strategy is a player's best move no matter what everyone else does. Moreover, the dominance of defection in a multiperson prisoner's dilemma holds regardless of the number of players: it is as true for two as it is for two thousand.[1]

However, it is also well known that defection is *not* the dominant strategy if the situation is repeated over time, as are many collective-action problems. Indeed, in the iterated prisoner's dilemma, there is no dominant strategy so long as the future is sufficiently important. In the repeated context, interdependence is paramount: what is best for player 1 depends on player 2's moves and vice versa. And again, this property holds regardless of the number of players. Even in enormous groups there need not be a dominant strategy, not even defection.

This last statement may occasion some surprise, but it is true. It is also consistent with Olson's formal analysis, for his model covered only the one-shot prisoner's dilemma. Although it is tempting to extend the conclusions outside the scope of his model, such extensions may be unwarranted. It is intuitively clear that all sorts of political arrangements are possible when there are repeat encounters, arrangements that may be impossible in one-shot affairs.

Most importantly, it is possible to sustain cooperation in the iterated game, a point noted by political scientists (Axelrod 1984; R. Hardin 1982; Taylor 1976) and economists (Friedman 1971; Radner 1980, 1986). The basic idea in all these works is conditional cooperation: offering stick-and-carrot combinations of rewarding cooperation with cooperation and punishing defection with defection. So long as one's partner does not discount the future too heavily, such strategies can secure the cooperative outcome because the short-term gains of double cross will be outweighed by the long-term costs of being mired in mutual noncooperation. Hence, joint cooperation is a Nash equilibrium, that is, holding fixed the strategy of one player, conditional cooperation is the other's best response. Thus, repetition matters a great deal.[2] The one-shot game has only one Nash equilibrium—joint defection; the iterated game has many equilibria, including cooperative ones.

This benign potential of repetition has led students of the prisoner's dilemma to reconsider the necessity of centralized solutions. Hobbes ([1651] 1968) and his intellectual descendants such as Garrett Hardin (1968) have seen the authority of government as the key remedy to the shortcomings of decentralized choice. Because everyone may want the collective good supplied or, as in the "tragedy of the commons," protected, an imposed policy may win unanimous support. However, in the setting of the repeated game the centralized solution may be unnecessary (Axelrod 1981, 316; Taylor 1976, 9–12).

Yet despite the potential of decentralized strategies of reciprocity to enforce cooperation, these scholars doubt that the argument can be extended to the large numbers case. As Taylor comments in a subsequent book,

> Unfortunately, [voluntary cooperation] is less likely to occur in large groups than in small ones, since a conditional cooperator must be able to monitor the behavior of others in the group so as to reassure himself that they are doing their parts and not taking advantage of him. Clearly, as the size of the group increases, this mutual monitoring becomes increasingly difficult and the "tacit contract" of conditional cooperation becomes increasingly fragile. (1982, 53)

Axelrod (1986) and Russell Hardin (1982, 53) have related reservations, as does Olson (1982, 23–35).

These doubts are buttressed by evidence. Robyn Dawes, reviewing a large body of experimental work, notes that "all experimenters who have made explicit or implicit comparisons of dilemma games with varying numbers of players have concluded that subjects cooperate less in larger groups than in smaller ones" (1980, 186). Researchers studying reciprocity in the field usually describe dyadic ties or small groups (Schmidt, Scott, Lande, and Guasti 1977, passim). The restriction of strategies of conditional cooperation to the small numbers case appears to be a robust empirical regularity.[3]

These theoretical speculations about the fragility of conditional cooperation in large groups, as well as the evidence indicating that it is most common in small n settings, suggest that a close inspection of the repeated game leads us full circle back to Olson's main conclusions. His static analysis of the difficulties confronting large groups seems to have a dynamic equivalent. Though at first glance iterated encounters appear to solve the dilemma, in real organizations the problems of monitoring, of misperception, of uncertainty about the underlying strategies of one's colleagues, cast doubt on the ability of decentralized strategies of reciprocity to stabilize cooperation.

Our aim in this paper is to formalize these intuitions about the fragility of cooperation in the repeated game involving large numbers and to show that this instability has implications for the institutional structure of interest groups and other organizations striving for collective benefits. Our analysis is game-theoretical. We represent the problem of collective action as an n-person repeated game under uncertainty. We do so not to introduce mathematical complexity for its own sake but to capture the mundane yet vital problems faced by political organizations.

To provide a baseline of comparison, we first analyze the simple context of perfect monitoring. Here we contrast a decentralized structure, where everyone uses trigger strategies of conditional cooperation, with a centralized regime that, in the manner of Olson's by-product theory, selectively administers inducements and punishments. In this baseline section the decentralized regime fully exploits the advantages of repeated encounters, uncontaminated by informational problems.

In the second section we show that,

once one introduces some realistic monitoring problems, Olson's insights into the difficulties facing large groups can be sustained in the context of repeated play. The model reveals that monitoring imperfections and causal uncertainties degrade the value of decentralized strategies in large groups. Simultaneously, such informational problems enhance the relative performance of the structure that centrally administers selective incentives—much as Olson had argued in his by-product theory of interest group viability.

Most importantly, we show in the third section that these problems of monitoring and inference can also give rise to a federated or nested institutional structure (Simon 1969) for very large groups. We take up two types of nested structures. The first structure combines decentralized reciprocity strategies at the local level with coarse selective penalties administered by a central office. Hence, the nested structure fuses the advantages of conditional cooperation (anarchy) and centralized authority (the state). By so doing, it illustrates that the repertoire of institutional forms is much richer than the classical pair of anarchy and the state on which so much attention has been focused. Via the second structure we explore a hypothesis of Russell Hardin that a federation of local units may sustain cooperation in a decentralized manner, without any central authority at all (1982, 196–197).

Perfect Monitoring

For concreteness, we interpret our organization as an interest group lobbying for a collective benefit. Each member of the organization can either work hard (cooperate) or slough off (defect). For simplicity, we normalize the effects of each agent's choices on output: if a member participates by working hard, then one unit of output is created; if the member shirks, no output is created. Since this perfect monitoring case is deterministic, the relation between effort and output is certain; every member can guarantee a contribution of one or zero. We assume that no one directly observes the effort of anyone else but everyone observes the collective output perfectly. Further, everyone knows the individual and organizational production functions. These latter assumptions allow everybody to make inferences about effort, that is, to infer what proportion of the organization was working hard and to do so without error. (Note, however, that the identity of shirkers cannot be ascertained.)

Collective benefits increase linearly in the contributions of the members, at a rate of b per unit of output. A member who participates bears a personal cost of c. For simplicity we assume that everyone is risk-neutral, so utility is linear in net benefits. If k_i denotes whether member i is working ($k_i = 1$) or shirking ($k_i = 0$), the resulting utility for that member is

$$U_i = \left(\frac{b}{n}\sum_{j=1}^{n} k_j\right) - ck_i \qquad (1)$$

This implies that all members of the group face the same strategic situation, an assumption that we will drop shortly.

If everyone cooperates, the output will be n, and the gross benefits will be bn. Thus for a divisible good the payoff equation (1) indicates that under full cooperation every member will reap net benefits of $bn/n - c = b - c$. To ensure that the situation is a prisoner's dilemma, we assume that

$$b > c > \frac{b}{n} \qquad (2)$$

Thus everybody is better off if everybody cooperates ($b - c > 0$), but in the one-shot game it is a dominant strategy to defect since the personal gain of participation b/n is smaller than the private cost c.

Note that the benefit share of each person—b/n—allows us to interpret the collective good as one characterized by nonrival consumption (a pure public good), in which case b/n denotes the productivity of a person's contribution. In the context of a pure public good, the productivity of each individual falls as the group grows. In the context of a collectively produced good exhibiting rival consumption, productivity can be independent of group size, but the benefits must be shared with more people. In either case b/n falls as the organization grows larger. To simplify the exposition, we will always refer to the case of rival consumption, wherein the per capita benefit of a unit of output is always b/n.

Condition (2) ensures that the group is large enough that rival consumption or decreasing marginal productivity of individual effort guarantee that each member's choice is a prisoner's dilemma. The result, in the one-shot game, is that everybody shirks and no collective good is provided. In the repeated game, however, collective defection is no longer the only equilibrium.

In the repeated game each person's utility is just the discounted sum of his or her utilities in the basic game. We assume that in every period there is a constant probability that the game will end or, equivalently, that the game goes on forever but the members discount the future. In either case, each member maximizes the discounted sum of his expected utility; if $U_{i,t}$ denotes the expected utility of player i in period t, i's objective is to maximize

$$W_i = U_{i,1} + \delta U_{i,2} + \delta U_{i,3} + \ldots$$

$$= \sum_{t=1}^{\infty} \delta^{t-1} U_{i,t},$$

where the parameter δ captures both the probability of termination and the players' time preference. Naturally,

$$0 < \delta < 1$$

In the context of a repeated game, it is well known (see, e.g., Friedman 1971) that, so long as the members do not discount the future too much, an appropriate trigger strategy will produce enduring cooperation by all members. The strategy is simple. With a group of size n, if everyone works hard the output will be n every period. Accordingly, the strategy is to adopt the following cutoff rule: if the output falls below n in any period, do not participate for the next T periods. With this rule, punishment is triggered if even one member sloughs off. Note that since the group's payoff is a deterministic function of individual effort, punishment is never invoked erroneously: the inference that output fell below the maximum because someone was free riding is perfect. The Appendix proves the propositions that follow.

Proposition 1

If relative to group size n the punishment phase is sufficiently long and the members do not discount the future very heavily, there is an equilibrium in which all members cooperate every period. However, given the discount rate, there is an upper size limit n beyond which the cooperative outcome cannot be upheld.*

This outcome is a Nash (best response) equilibrium, as it will be in all subsequent propositions.[4]

The length of the punishment phase, T, depends upon how much the members discount the future: the less the future matters, the longer punishment must last to support the cooperative outcome.[5] More specifically, T must be large enough so that

$$\frac{1 - \delta^{T+1}}{1 - \delta} \geq \frac{b - b/n}{b - c}, \tag{3}$$

and δ must be large enough so that

$$\delta \geq \frac{c - b/n}{b - b/n} \tag{4}$$

Rearranging the righthand side of (4) to yield $nc-b/nb-b$ is instructive, for it shows that this term is increasing in n, the group size. Thus for any fixed δ there is a critical threshold on group size, n^*, beyond which cooperation cannot be sustained. The value of this critical size is a function of the costs and benefits of participation, as well as of the discount parameter.[6] The intuitive reason for this effect is as follows: The benefit of cooperation in any period is independent of group size. Each member obtains a payoff of $(b - c)$ if everyone participates; on the other hand the short-run gain to a member by shirking $(c - b/n)$ is higher in larger groups. Consequently for a given δ, the loss from suspended cooperation in later periods $(\delta(b-c)/1-\delta)$ is independent of group size n and for large enough groups is insufficient to deter members from realizing the short-term gains from shirking.

Before moving on to the centralized regime, we wish to generalize this result. The generalization is interesting in its own right and will prove useful in later sections of the paper. Typically, the prisoner's dilemma is analyzed under the assumption of symmetric payoffs, and proposition 1 used that assumption. This condition is often not satisfied empirically. For many reasons, participation is more costly for some people than for others. For example, in all interest groups some members have more political skill, some less. Because ability and effort are partial substitutes, the politically inept must exert more effort to generate the same contribution. Thus, cooperating imposes higher private costs on the low-ability members.[7] Though they face higher private costs, we assume that even the low-ability members face a prisoner's dilemma: they, too, are better off if everyone contributes than if no one does. Thus we have two levels of c satisfying

$$b > c_1 > c_2 > \frac{b}{n}$$

We assume that one's ability is private information, though everyone knows the distribution of ability in the population at large. Then provided that conditions (3) and (4) hold with c replaced by c_1, it is straightforward to prove the proposition that follows.

Proposition 1a

If the punishment phase is sufficiently long and members do not discount the future too heavily, there is an equilibrium in which both low and high cost members cooperate.

It is important to note that both propositions cover large groups, subject to the qualification (noted above) that the discount factor be sufficiently large relative to group size. With both symmetric and asymmetric payoffs, completely decentralized strategies can produce the organizational equilibrium of full cooperation. This is not particularly intuitive. Hardin, for example, doubts that decentralized strategies will work in very large groups (1982, 40). We agree. The point of propositions 1 and 1a is twofold: first, to exhibit conditions that suffice for supporting a decentralized, cooperative equilibrium; second, to help us examine the robustness of the result. Note that the informational conditions are very restrictive. Everyone observes the group outcome *without error*, and everyone knows that the relation between effort and outcome is deterministic. Together, these two conditions guarantee that everyone knows that payoffs can fall only because of deliberate shirking. There are no external disturbances that could unpredictably reduce rewards. Consequently, the group need not worry about mistakenly triggering a punishment phase. Accordingly one cannot make the trigger strategy too draconian, for the punishment phase will never occur in equilibrium.

In such an informationally simple world, Hobbesian solutions of centralized authority are unnecessary. However, to provide a baseline of comparison for later sections, we introduce a centralized regime. In this arrangement, the members of the interest group establish an association with a central office or headquarters. Headquarters is given the authority to monitor the members and to expel shirkers from the association.[8] Because shirkers are no longer members in good standing, they cannot receive the benefits that accrue to members of the association.

Thus, this regime presumes either that the good in question is excludable—not a pure public good—or, as in Olson's by-product theory, that the organization has recourse to auxiliary incentives that can be selectively withheld from shirkers, in amounts calculated to offset the free collective good enjoyed by a defector. As Olson noted, these selective incentives may be only loosely related to the manifest political purposes of interest groups (e.g., legal insurance provided at lower rates for AMA members); their essential feature is that they can be allocated in a discriminating manner. Naturally, organizations that can neither expel members nor selectively withhold auxiliary benefits have a smaller strategic repertoire, for their only feasible strategies involve imposing rewards and punishments diffusely on the entire group, as in the trigger regime described earlier. Though propositions 1 and 1a indicate that recourse only to diffuse schemes is not a problem when all inferences are certain (and the group is below the critical size n^*), it seems plausible that in the real world this limited repertoire would impair group performance, much as Olson argued. We shall show below that there are good reasons for believing this to be so.

We assume there is a fixed probability q that a member will be monitored in a given period. This probability is independent over time. If the member is observed and found to be shirking, he or she is expelled and randomly replaced by someone from the outside population.[9] One might imagine a queue of outsiders who desire admittance into the group. Being expelled yields the same reward as when everyone free rides. Expulsion is permanent.

In such a regime the intensity of monitoring—the value of q—is clearly pivotal. If monitoring is very good (q sufficiently close to 1), everyone will participate since by assumption both low- and high-skill members are better off contributing than being thrown out. If monitoring is very bad, no one will participate. The interesting circumstance occurs when q takes on intermediate values. The difference between low and high skill can then matter a great deal, particularly if the private costs of participation vary significantly. Because members with little political ability incur high private costs of contributing to the group's lobbying campaign, it seems plausible that they might be tempted to shirk as long as they were unlikely to be caught. High-ability members on the other hand might not risk expulsion. This intuition is correct.

Proposition 2

Assume that the two types of members experience sufficiently different private costs of participation and that the probability of detection is intermediate. Then if there are enough high-ability types, there is an equilibrium in which all low-ability members free ride and all high-ability members contribute.

Note that in such a world, observing a member shirking even once reveals with certainty that he or she is of low ability. Since such members will never contribute to the common weal, all other members will agree to expel the deviant forever, for there is a chance that the replacement will have high ability and will participate. Accordingly, the policy of permanent expulsion is sensible.

The relevant range for the monitoring probability q is

$$\frac{1-\delta}{\delta}\,\frac{c_2 - b/n}{b(1-f) - c_2} < q < \frac{1-\delta}{\delta}\,\frac{c_1 - b/n}{b(1-f+1/n) - c_1} \qquad (5)$$

This condition expresses a simple property: for high-cost participators, the possibility of being observed must be small relative to the benefit-cost ratio of defecting versus contributing ($c_1 - b/n$ versus $b(1 - f + 1/n) - c_1$, respectively); for low-cost contributors, the opposite must hold. The private costs of participation of the two types of members are sufficiently disparate if the following inequality is satisfied:

$$b > c_1 > \frac{b}{n}L_1 > \frac{b}{n}L_2 > c_2 > \frac{b}{n}, \qquad (6)$$

where

$$L_1 = \frac{1-\delta+\delta q+\delta qn(1-f)}{1-\delta+\delta q}, \text{ and}$$

$$L_2 = \frac{1-\delta+\delta qn(1-f)}{1-\delta+\delta q}$$

If $(1 - f)$, the fraction of hard-working high-ability members in the group is high, membership in this group is valuable. Condition (6) states that the risk of being expelled from such a successful group is sufficient to deter high-ability individuals from shirking but not the low-ability members.

Note how this proposition contrasts with proposition 1a. Whereas the decentralized trigger strategy enforces participation from everyone, the centralized regime allows some members to free ride. Proposition 2 accords with the folk wisdom about the distribution of work in organizations: a fraction of the population does most of the work. Of course, if the probability of detection is very high, no one will risk shirking, and the two kinds of solutions will produce the same result of universal cooperation. But even then, there is no positive reason to turn to the Hobbesian solution. Thus, under the informational conditions described above—flawless observation of group payoffs and individual contribution a known function of individual effort—the case for centralized authority seems fairly weak.

Decentralized and Centralized Structures with Imperfect Monitoring

It is unlikely that such pristine informational conditions obtain in many interest groups. Therefore let us now introduce the merest smidgen of uncertainty in the relation between members' efforts and group performance (real or perceived). For instance, a member may try but fail to contribute to group performance because of an obstacle beyond the member's control. A person may be unable to come to an important meeting, despite best intentions, because of a sudden crisis at home. Or, despite everyone's contributing, group performance may be misperceived to have been inadequate.[10] In this section, we shall examine the consequences of very small errors of perception or external uncertainties and argue that the relative performance of the decentralized and centralized structures may alter dramatically compared to the world of complete certainty.

The environmental disturbances or monitoring errors that arise in real organizations are, typically, imperfectly correlated across different members. Disturbances can be purely idiosyncratic, affecting the link between effort and contribution of a single member. Or they can be systematic, for example, a surprisingly sharp recession that simultaneously impairs the well being of everyone in an interest group. The degree of correlation can thus vary greatly. However in what follows we shall consider only the case of idiosyncratic disturbances.[11] Thus, we shall assume that a member's participation, instead of *guaranteeing* a high perceived contribution, will do so with some probability $\bar{p}$. With probability $(1 - \bar{p})$, which we shall take to be small, a member's hard work comes to naught in that period. On the other hand, free riding continues to guarantee a contribution of zero.[12]

The introduction of this tiny bit of uncertainty opens a wedge between collective output and individual effort. With the rest of the informational assumptions unchanged, it is now possible that output will fall below n despite everyone's working hard. The problem is that such a shortfall could also be due to free riding. To deter individuals from shirking in the decentralized regime, they have to retain the noncooperative phases if group performance is deemed inadequate. Because such punishment phases will occasionally be triggered off by mistake, the expected utility of a representative member will be reduced relative to an error-free world of perfect inferences. How far it falls obviously depends on the probability that group performance falls below the commonly agreed-upon cutoff, despite maximal effort from all members.

This probability of breakdown is sensitive to two factors: (1) the cutoff level demarcating adequate from inadequate group performance and (2) the size of the interest group. The latter factor is important because of the independent nature of uncertainties in the contributions of different players. For example, if the cutoff level is maximal group performance n, group output will clear this with everyone participating only if *everyone* contributes successfully. The probability of this occurring is $\bar{p}^n$, which drops surprisingly fast as n increases. For example, if n is 50—a rather small interest group—and $\bar{p} = .99$, then the probability that everyone contributes successfully is only .605. Consequently, in every period there is almost a 40% chance of triggering a punishment phase of universal shirking, even though no one was at fault. Large groups may respond to this problem by lowering the cutoff level, a point we shall address shortly.

If even intense effort by all concerned is so likely to lead to a breakdown of cooperation, one may doubt whether it is feasible in large groups to provide members with adequate participation incentive. However, it is possible to show that if the uncertainty at the individual level is small, then such incentives can be adequately provided by the threat of reversion to noncooperative phases.

Proposition 3

Assume that individuals do not discount future payoffs too much, in the sense that condition (4) holds. Then for a given group size, if the exogenous idiosyncratic uncertainty $(1 - \bar{p})$ is small, there is a trigger strategy equilibrium where each member is motivated to participate in normal periods.

However for any given rate of discounting and size of the group, there may exist many trigger strategy equilibria corresponding to different choices of the performance cutoff level (denoted by C_n for an n-size group) and the length of punishment periods (denoted by T_n). It is important to note that choice of C_n and T_n are significant elements in the design of the decentralized organization; they specify the mutually agreed standard defining adequate group performance and the consequences of failing to meet this standard. The *feasible* alternatives for $\{C_n, T_n\}$ are those that induce all members of the group to participate in normal cooperative periods.[13] Further, both choice variables will affect the ex ante payoff to each member: C_n will define the probability of *breakdown of cooperation;* T_n, the number of periods of defection thereafter.[14] It can be shown that members of different abilities will rank different feasible alternatives $\{C_n, T_n\}$ identically, so the group would unanimously approve the cutoff level and punishment period that maximizes the ex ante payoff of a representative member, from among the feasible set. Because such a choice would express the best each member could expect from the decentralized regime, it is thus relevant in evaluating members' preferences between this and alternative organizational forms such as the centralized regime. It would thus be extremely convenient to derive a complete analytical description of the best choice of $\{C_n, T_n\}$ for any group size n. Such a result still eludes us. We can, however, describe some of the properties of the best trigger strategy equilibrium.

Proposition 4

(1) *The best cutoff level C_n for a group of size n must be less than $n\bar{p} + 1$, where $n\bar{p}$ is the expected level of group performance when all members participate; (2) if for a given cutoff level C_n, the threat of a T-period punishment phase motivates all members to participate in a normal period, then the threat of any longer ($T' > T$) punishment phase also motivates them but generates a lower expected payoff for every member.*

The first part of this proposition says that the cutoff level should not be significantly larger than the expected group performance in a cooperative period. The intuitive reason is that cutoff levels much higher than the mean make a breakdown of cooperation too likely and can be lowered while preserving participation incentives. But if the cutoff level is smaller than or equal to the mean, lowering it may destroy incentives. The second part of the proposition implies that if a particular cutoff level C_n can feasibly provide participation incentives (i.e., combined with *some* T-length punishment phases), the group should choose the smallest T that provides these incentives in conjunction with C_n.

Given only such a limited analytical characterization we shall be forced hereafter to concentrate on some numerical examples where we can explicitly calculate the payoffs group members can get from the best trigger strategy equilibrium. Such payoffs will be compared to what they can get from the centralized regime. Before we do this, we need to verify that with small idiosyncratic uncertainties, the centralized regime will be characterized by behavior similar to that in the complete certainty context.

Proposition 5

Assume (5) and (6) hold and that members detected shirking in the centralized regime are permanently expelled. Then if the idiosyncratic uncertainty $(1 - \bar{p})$ is small, there is an equilibrium in the centralized regime where every low-ability member always shirks and every high-ability member always participates. Further, the ex ante payoffs in this equilibrium to every member can be explicitly calculated.

Note that with small uncertainties $(1 - \bar{p})$ in the contribution of a participating member, a given high-ability member may accidentally generate a low contribution and be expelled permanently with probability $q(1 - \bar{p})$.[15] Thus the expected payoff of a high-ability member is lower than in a world of no uncertainty or error. However, what is of interest is this payoff *relative* to the best members can expect from a decentralized trigger strategy regime.

In a numerical example we have computed the best payoffs players can expect in the decentralized structure when the chance of an idiosyncratic disturbance is

Table 1. Performance of the Decentralized Regime under Imperfect Monitoring

Group Size	Cutoff (C_n)	Shortfall Allowed	Punishment Period (T_n)	Probability of Collapse	Expected Payoff, High Ability	Expected Payoff, Low Ability
25	24	1	2	.026	4,552	3,793
50	48	2	5	.014	4,522	3,768
75	72	3	20	.007	4,506	3,755
100	97	3	7	.018	4,374	3,645
125	122	3	4	.037	4,258	3,549
150	146	4	11	.018	4,277	3,564
175	171	4	6	.032	4,185	3,487
200	195	5	20	.016	4,218	3,515
225	220	5	10	.027	4,110	3,425
250	245	5	7	.041	3,982	3,318
275	269	6	19	.022	4,062	3,385
300	294	6	11	.033	3,952	3,294
325	319	6	8	.047	3,832	3,193
350	343	7	21	.026	3,931	3,276
375	368	7	12	.037	3,839	3,199
400	393	7	9	.050	3,721	3,101
425	417	8	26	.029	3,818	3,181
450	442	8	15	.039	3,707	3,089
475	467	8	11	.052	3,596	2,996
500	491	9	49	.031	3,717	3,099
1,000	986	14	22	.082	2,848	2,373
1,500				not sustainable		

one in a hundred. In Table 1 we show how the decentralized structure performs in groups ranging from 25 to 1,500.[16]

The table reveals two important patterns. The first was anticipated: the larger the group, the smaller both average payoffs tend to be. There are some minor exceptions, but the overall decline is clear. For example, in a group of 1,000 the payoff of a high-ability member is 62% of the payoff's value in a group of 25. The centralized structure—which is insensitive to size—gives high-ability members a payoff of 4,289, thus beating the trigger strategy regime in groups of 125 or more. The deterioration of the decentralized structure is due to the increasing fragility of decentralized cooperation; other things being equal, the larger the group the bigger the chance that an idiosyncratic disturbance will drive the group's performance below the cutoff. Thus in an organization of 1,000, there is in every period more than an 8% chance of an accidental breakdown of cooperation, compared with less than a 3% chance in the smallest group. And this occurs even though the large group reduces its fragility by tolerating larger shortfalls in performance, C_n being 14 below maximum performance when $n = 1{,}000$ but only one below the maximum in the smallest group.[17]

One might wonder whether the instability problem can be further ameliorated by lowering the cutoff levels even more than reported here. This, however, will not work, since doing so makes the group *too* likely to achieve the target level even if the n^{th} player deliberately shirks. Thus, cutoff levels smaller than Table 1's do not provide adequate effort incentives. Thus the decentralized structure faces a trade-off between increasing its stability and maintaining incentives for participation. Reducing the cutoff level enhances stability but it also eventually makes individuals too dispensable, destroying participation incentives.

Naturally, if the monitoring uncertainties are less pronounced ($\bar{p}$ closer to 1), the decentralized regime is less unstable and its performance degrades less quickly as size increases. Nevertheless, the qualitative pattern remains the same: even if the chance of a disturbance is only one in a thousand, size impairs performance. (The authors will provide a printout of results for the case of $\bar{p} = 0.999$ upon request.) Thus even in this nearly deterministic setting the centralized institution beats the trigger regime in moderately large groups, in particular those over two thousand members. As conjectured, the centralized regime is superior in large organizations.

The table's second important pattern is indicated by its last column. By doing calculations for *every* possible cutoff level, we found that no $\{C_n, T_n\}$ pair supports cooperation in a group of 1,500 members when $\bar{p} = 0.99$. This result generalizes.

Proposition 6

Given the level of uncertainty $(1 - \bar{p})$ in individual contributions corresponding to maximum effort, there is a critical group size $n\dagger$ above which there is no cooperative trigger strategy equilibrium.

This result is relatively intuitive. The equilibrium of the decentralized regime requires that if $n - 1$ members cooperate, it must be sufficiently likely that the n^{th} member will be pivotal, that is, without his or her contribution, the group's performance will fall below the cutoff. The probability that the n^{th} member is pivotal is just the probability that the rest of the organization generates an output of $C_n - 1$. Because the binomial distribution flattens out for large numbers of trials, this probability falls arbitrarily close to zero for sufficiently large n. Hence the likelihood that any individual will be pivotal becomes vanishingly small in very large groups, and the decentralized regime can no longer sustain cooperation.[18]

Note that this upper-bound $n\dagger$ on the feasible size of groups is quite distinct from the bound of n^* described in the previous section. The former arises from the probabilistic character of conditional cooperation under uncertainty. The latter, which simply expresses the relative benefits of defecting now versus the costs of foregoing future cooperation, holds under the deterministic conditions of perfect monitoring and has no probabilistic content. Indeed, under the parametric conditions of Table 1, n^* equals infinity—and hence is not a binding constraint—whereas $n\dagger$ does limit the feasible size of groups.[19]

Federal Structures

In the previous section we have seen that in the presence of even small uncertainties or perceptual errors, both the purely decentralized and centralized solutions to the collective action problem may exhibit drawbacks. The decentralized structure based on trigger strategies collapses too often; the centralized regime may tolerate some free riders. When this happens, one naturally wishes to explore alternative institutional forms that might perform better. In particular, are there alternatives that are more stable than the trigger strategy regime, as well as less burdened by free riding than the centralized one?

The instability so characteristic of the trigger strategy structure is rather similar to the kinds of instability problems discussed by Herbert Simon (1969). Simon argued that the ubiquity of uncontrollable disturbances skews the evolution of organizational structures toward those that localize the effects of such shocks. He hypothesized that hierarchic forms—in the sense of a nested structure of units within units—provide the desired type of protection. So we now consider a hierarchical or federated structure for the interest group. The group divides itself up into a number of local chapters; it also retains a central office. Instead of monitoring the performance of individuals, the central office monitors the collective performance of local chapters. To maintain comparability across organizational forms, we assume headquarters has the same monitoring resources as in the fully centralized expulsion regime. Because it is monitoring fewer units—chapters instead of individuals—the probability that any chapter will be observed is proportionately higher. For example, if the group has one hundred members, with $q = 0.01$ in the expulsion regime, then with 10 chapters, each one has a 0.1 chance of being observed. Thus we assume that in an n-sized group comprising m local chapters with $j = n/m$ members in each chapter, the central office audits the performance of each chapter with probability jq.[20]

A number of variants of the federal structure are possible, depending on the nature of the collective good and the instruments of control available to the central office. We first describe a context where the collective good exhibits rival consumption or has the quality of a local public good. Thus if someone is suspended from the organization, that person cannot enjoy the collective benefit for the relevant time period. (Consider, for example, a disbarred lawyer, or a union local denied strike funds by the national organization.) In this setting we will analyze a federal structure that uses both decentralized and centralized strategies: the former at the local intrachapter level and the latter between headquarters and individual chapters. Regarding the latter, the central office audits the performance of any chapter of size j with probability jq. If in any period this is observed to fall below the threshold level bj, all members of that chapter are suspended from the organization *in that period.* In such a period these members get a payoff of zero. If chapter performance is observed to be at the required level, each member receives the normal benefits of membership, b. In periods when a chapter's contribution is unobserved, the center treats those members exactly as it did the last time it monitored that chapter. Thus if at any date the chapter's performance is found wanting, members are suspended until the next time headquarters observes their collective performance and verifies that it satisfies the required level.

Within every chapter, members employ a decentralized trigger strategy. They start by cooperating—even the low-ability ones. But if at any date a chapter is found inadequate by headquarters and suspended, every member will shirk for the next T periods, thereafter returning to the strategy of conditional cooperation. Given the strategy of the central office, this chapter will remain suspended for the entire all-shirk phase because throughout this time it will contribute nothing. Even after the chapter's noncooperative phase ends after T periods and it resumes contributing at level bj, it may remain suspended if it so happens that headquarters does not monitor its performance. The next proposition shows that if each chapter is of a certain minimum size, these trigger strategies form an equilibrium pattern of behavior.

Proposition 7

Assume that the interest group is federated into m subgroups with $j = n/m$ members in each subgroup and that the collective performance of each subgroup is monitored with probability jq every period. Suppose that the central office uses the strategy of suspending all members of a subgroup in any period when its performance is observed to fall below bj, until the next time it discovers the subgroup's performance is equal to the *desired level bj. In all other periods, each subgroup member obtains a benefit level b of the collective good. In this regime if each subgroup has a minimum size j given by the condition*

$$j > \frac{1}{q}\left(\frac{1}{\delta} - 1\right)\frac{c_1}{b - c_1} \qquad (7)$$

and if the idiosyncratic uncertainty $(1 - \bar{p})$ *is small, there exists an equilibrium where each member of the subgroup chooses the following trigger strategy: participate (i.e.,* $p = \bar{p}$*) at date 1 and thereafter unless at date t the center suspends the subgroup for inadequate performance, in which case shirk for the next T periods. The expected payoff to each member from this equilibrium can be explicitly calculated.*

In contrast to the purely centralized regime based on monitoring individuals, the federal regime is beset by no free riding. In this nested structure, all members participate in cooperative periods because a failure in individual performance causes the chapter's performance to fail, thus inviting punishment with probability jq. In contrast, an individual's failure in the centralized regime invites punishment with the lower probability q. Even though punishment in the federal regime is a temporary suspension rather than permanent expulsion, if jq is large enough the federal structure provides better participation incentives than the centralized regime. This translates to a condition that, given q, the size j of the subchapter must be large enough—which is expressed by (7)

While the federal structure's use of decentralized trigger strategies eliminates free riding, it does not create the degree of instability characteristic of a purely deccentralized (unfederated) structure because in the federated structure the trigger strategies are only used locally, within chapters. This contains the instability arising from uncontrolled disturbances or monitoring errors and does not threaten members of other chapters. Thus the overall organization is protected from local shocks. Given that decentralized strategies generate more instability, the larger the relevant group, it is clear that each chapter must not be too large if a federated structure is to outdo the unfederated one. This contrasts with the requirement of (7) that to prevent free riding each chapter not be too small.

Hence we must check the feasibility of federated structures. Are there chapters small enough so that the nested organization is significantly more stable than the purely decentralized structure but large enough to ensure there is less free riding than in the purely centralized system? In the example considered in the previous section, condition (7) requires each chapter to have at least 3 members. Even if we consider chapters with 10 members each, a typical member obtains expected payoff $(4{,}514.5 - 9.1c_i)$ from the federal structure that exceeds the payoff to high-ability members in the centralized regime. Thus, in this example, whenever the group is large enough for the centralized organization to yield higher payoffs than the purely decentralized regime, the federal structure dominates them both.[21] However, this should not be taken to mean that some federated structure invariably dominates the two unfederated regimes. For instance, if monitoring in the centralized system is good enough, it may not be burdened by any free riding at all. In that case it could dominate any federated institution of the type discussed so far by reducing the risk of being suspended or expelled by the center. The point is that if the centralized structure is characterized by free riding (which it will with sufficient dispersion in private costs of participation), then a federated form can improve on it, as well as on the purely decentralized structure if the overall group is large.

So far we have discussed the context of

a local public good that members can be temporarily suspended from enjoying. However, many interest groups such as lobbies typically cannot bar shirking members from enjoying the benefits of a piece of legislation they help pass. In such circumstances interest groups may still be able to punish members selectively if they have additional instruments of control like those Olson argued were important in solving collective action problems in large groups. A previous version of this paper describes variants of the federal structure that can be employed in this case (Bendor and Mookherjee 1985, prop. 7).

Finally, we wish to point out that other variants of the federal institution may be useful in different contexts. The structure described in proposition 7 uses centralized monitoring and punishments of subunits, along with decentralized strategies within chapters. It thus relies on centralized authority to enforce punishments. Alternatively, Russell Hardin has suggested that large groups without any internal authority structure at all may be able to resolve collective action dilemmas by using a federated structure (1982, 184). He argues that despite the absence of a central authority, subunits may be able to regulate themselves via decentralized strategies. Such self-regulation could arise if there were multiple activities going on simultaneously in each chapter. For instance, the existence of subunits inevitably promotes social interaction among members so that task-oriented and social activities coexist. Hardin argues this enables local chapters to regulate themselves for two reasons. First, multiple activities provide everyone with multiple opportunities for judging the character and preferences of peers. The significance of maintaining a good reputation is thus enhanced. If a colleague plays dirty in one setting, one might suspect that person of free riding in another. Second, multiple games imply multiple instruments of control. Even if the group's instrumental actions are aimed at securing a pure public good that cannot be denied to free riders, the social activities provide potential sanctions that can be adjusted to a fine tolerance.[22]

We now formalize the second idea—the presence of multiple instruments created by multiple overlapping activities—to show how a federated structure relying on decentralized strategies alone can overcome collective-action problems. Assume that members interact (1) globally by contributing to the organization as a whole and (2) locally by participating in social activities with people in the same chapter. Both global and local games are subject to a prisoner's dilemma.[23] A member k of subgroup g must choose p_k and e_k, contributions to global and local activities respectively. As before, the global collective good is subject to monitoring uncertainties. The local activities are, however, perfectly observed—a simplifying assumption that is quite inessential. Thus while p_k has to be chosen between 0 and $\bar{p}$, e_k can be chosen between 0 and 1. The utility of member k in any period is then

$$U^k = \frac{b}{n} \sum_{g=1}^{m} \sum_{i \epsilon I_g} p_i + \frac{\beta}{j} \sum_{i \epsilon I_g} e_i - c_k(p_k + e_k), \tag{8}$$

where

$$b > c_k > \frac{b}{n} \text{ and } \beta > c_k > \frac{\beta}{j}, \tag{9}$$

and I_g denotes the set of members of chapter g.

Proposition 8

Assume (8) and (9) hold and also that

$$\delta(\beta - c_1) \geqslant \left(c_1 - \frac{b}{n}\right) + \left(c_1 - \frac{\beta}{j}\right) \tag{10}$$

Then if $(1 - \bar{p})$ is small, there exists an *equilibrium with the following properties: 1) all members always choose global participation $\bar{p}$; and 2) local participation is maximal ($e_i = 1$) unless at the previous date either the chapter's collective contribution to the global organization or the scale of local social activity fell below bj, in which case $e_i = 0$.*

Here inadequate subgroup performance is discouraged by the threat of defection in the local game. Members never defect on their contributions to the global organization, thus stabilizing the latter. Condition (10) describes what is required for this to be an equilibrium: the short-term benefits from shirking in both global and local activities must be outweighed by the prospect of a collapse of local cooperation in the next period. The existence of overlapping local benefits and the prospect of a breakdown of cooperation in these games provide an alternative to the selective punishments administered by a center in the previous regime of proposition 7.[24]

However, we must emphasize that this method of providing incentives to contribute to the global cause is vulnerable to local collusion. A chapter can profitably free ride on other subgroups' contributions to the global organization, simultaneously cooperating on local activities. Such forms of local collusion are especially likely in large organizations where local units are alienated from the rest.[25] While the foregoing analysis upholds Hardin's point that it is *possible* for very large groups to organize collective action in a completely decentralized fashion in the presence of overlapping local interactions, the prospect of local collusion may also explain why most examples of such forms of collective activity are of mass movements wherein individuals strongly identify with the main cause, thus reducing the chance of local collusion.

Naturally, other chapters can try to deter collusion in a particular chapter. But they must tread carefully. If they threatened to withdraw their own contributions, then in effect they would have returned to the completely decentralized regime, whose drawbacks we know well. Alternatively, the other chapters could selectively sanction the deviant subgroup, thereby returning to the dual institution of proposition 7. We believe that large groups that initially rely on completely decentralized, self-regulating subunits will either work their way toward dual regimes or will suffer pronounced instability as the subunits withdraw effort in retaliation for real or perceived shirking.

Conclusions and Implications

Since Hobbes, problems of collective action have fascinated and troubled political scientists. In the main, this interest has resulted in a concentration on two polar solutions: cooperation sustained by decentralized strategies of reciprocity (anarchy) and cooperation enforced by central authority (the state). These alternatives certainly merit our attention. They do not, however, exhaust the set of institutional forms that can ameliorate problems of collective action. We believe that this focusing on the polar pair of completely decentralized and completely centralized solutions has led us to overlook an important class of organizational forms: nested structures that combine strategies of conditional cooperation at local levels and strategies of selective incentives at the global level.

This class is not the pipe dream of institutional designers; it already exists. In fact, it is a robust empirical regularity. Several generations of researchers in industrial sociology and organizational behavior have concluded that every formal organization is laced with networks of informal exchange and reciprocity.[26] Even in the tightly controlled settings of prisons, decentralized strategies of conditional cooperation appear spontaneously.[27]

Yet their scope is limited. Reciprocity strategies are generally confined to the small numbers case: a small work unit, a legislative faction, a union local. Typically, a formal structure involving centralized means of eliciting and enforcing cooperation links the small subunits. This, too, is a robust empirical pattern (Meyer 1972; Starbuck 1965).

Of course, local strategies do not always reinforce global ones. Often they conflict. Industrial sociology is filled with case studies of how work groups informally collude against the headquarters of a firm, and there are numerous accounts of similar "departmentalitis" in the Soviet bureaucracy (Granick 1959). Therefore, it would be patently false to claim that the mere existence of dual strategies indicates that a group has solved its problems of collective action. Instead, our point is that to the extent that a large organization has ameliorated these problems, it may well have done so via a nested structure in which formal centralized strategies and informal decentralized ones reinforce each other. Further, this nesting is particularly important when inferential problems make the decentralized strategies destabilizing.

Conversely, when such federated systems are infeasible, Olson's conclusions reappear in a dynamic and probabilistic form. In large nonfederated groups that rely on decentralized strategies, cooperation is fragile at best. Iterated encounters raise the possibility of cooperation; given imperfect observation, iteration also implies repeated breakdowns. And in very large groups decentralized cooperation cannot be sustained at all.

Empirically, of course, nested structures are the rule. Among interest groups, it is virtually impossible to find a single one of any substantial size that does not exhibit a "Chinese box" form. Trade associations are built out of firms, themselves hierarchic systems; the American Medical Association has well-organized state associations; the AFL-CIO is nested many layers deep, with member unions organized into locals.[28]

Hierarchic structures are ubiquitous for many reasons. Clustering by subunits makes it easier for newcomers—especially new leaders—to understand an organization, thus easing problems of bounded rationality. The hierarchical decomposition of problems takes advantage of knowledge dispersed throughout an organization and economizes on communication and other information-processing tasks (Geanokopolos and Milgrom 1984; March and Simon 1958). It reduces transaction costs of intraorganizational bargaining, and, as we have emphasized here, it localizes the effects of exogenous shocks (Simon 1969).[29] The nested structure of large organizations may well be overdetermined. Regardless of the cause, large interest groups and other organizations will almost always have federal structures. The smallest subunits provide a natural breeding ground for decentralized conditional cooperation. This suggests that the real choice is not between anarchy and the state but between different forms of dual control.

Limitations

We see three significant limitations of our models. First, all of our results depend on the "shadow of the future" looming large enough. This has a subjective and an objective component. Subjectively, the members of a group must not discount the future too heavily, as we have often noted. Objectively, there must not be too much turnover in the organization. We have finessed the objective component by assuming that no one voluntarily leaves the group, no one dies, no one emigrates. How far cooperation will fall once these assumptions are relaxed is an interesting question. As is common in most repeated play models with farseeing strategic actors, it seems likely that it will be hard to prevent members near retirement from free riding. However, since most interest groups are composed of several overlapping generations, free riding by senior members need not lead to organizational collapse. Indeed, a pattern of contributing a lot when young and free riding when old may be an implicit social contract that is a Nash equilibrium (Cremer 1986).

Second, in the expulsion and federal regimes we have not explicitly modelled the behavior of the group's headquarters. The central office never shirks its duty; it always expels people when it discovers free riding. Moreover, headquarters never *exceeds* its duty either. Abuse of power never occurs; headquarters never manipulates the rest of the group for its own ends. Michels's "iron law of oligarchy" has been assumed away.

It would be interesting to relax these assumptions by modelling a repeated game between leaders and ordinary members. This game itself may be an iterated n-person prisoner's dilemma. Both sides might be better off abiding by the terms of a deal than if they both defect, but each side may do better still by defecting while the other side cooperates (e.g., the leaders by enjoying the perquisites of office while neglecting their duties).

The third limitation is perhaps the most important. Game-theoretical models such as ours can explain why certain patterns persist; they do not explain how they arise. The emergence of cooperation is a hard problem—one that may require other methods of analysis.[30] Of course, when there is only one Nash equilibrium in a repeated game, one might predict that will be the limiting outcome of many of the adjustment paths. To use the vivid language of Markov processes, such an outcome is an absorbing state. Once the system gets there, it stays there. And, in all likelihood, it will get there eventually.

However, in most repeated games there are multiple equilibria, often an embarrassment of riches. This is so for the iterated prisoner's dilemma. Consequently, an interest group may grope its way toward a cooperative equilibrium, sustained by intricate institutional forms wherein global inducements complement local reciprocity, or it may grope toward the "social trap" (Cross and Guyer 1980) of minimal cooperation, in which everyone could be better off but in which the prevailing codes rob every individual of reasons to exert any effort. Which history unfolds may depend on idiosyncratic events, for example, an unlucky early defeat that creates widespread suspicion of shirking. Consequently, explaining why patterns persist may be more amenable to theoretical analysis than may explaining how they arose.[31]

We suspect, however, that this issue of dynamics strengthens one of our conclusions: the purely decentralized regime is even less viable than we have suggested. A group that is trying to move toward a common strategy of conditional cooperation faces formidable coordination problems. During the disequilibrium of trial-and-error adjustments, members will use different cutoff levels and punishment periods of varying length. The resulting potential for confusion, for misperceptions that slow down the convergence towards an equilibrium institution, would be enormous. Indeed, an explicit examination of evolutionary paths may well reveal that nested structures, building as they do on "stable subassemblies" (Simon 1969), are even more important than our equilibrium analysis indicates.

Appendix

Proof of Proposition 1

To prove the first part, we show that if conditions (3) and (4) in the text are satisfied, then at any stage of the game the postulated trigger strategy is an optimal response for any given member i to the use of the same strategy by all other players. Within any punishment phase, choice of contribution affects the decision of no other player in the future, so it is optimal for i to choose $p_i = 0$. Outside punishment phases, the most i can obtain for the remainder of the game at any given date t by defecting is

$$b\left(1 - \frac{1}{n}\right) + \delta^{T+1}\frac{(b - c)}{1 - \delta} \qquad \text{(A1)}$$

since the remaining $(n - 1)$ members are choosing to cooperate at t, but cooperation will break down in periods $(t + 1)$ to $(t + T)$ and then start again from $(t + T + 1)$ onwards. By not defecting at t, i's payoff from the remainder of the game is

$$\frac{(b - c)}{(1 - \delta)}, \qquad \text{(A2)}$$

which is greater than (A1) by virtue of (3). That there exists an integer T satisfying (3) is guaranteed by (4).

To prove the second part of the proposition, note that the worst equilibrium from any member's point of view is the one where everyone defects perpetually as this gives each member a min/max payoff of zero. Hence, using results in Abreu (1982), perpetual cooperation can be supported as an equilibrium outcome if and only if unilateral shirking by single members is not worthwhile to them, provided such deviations are followed by the group reverting to the all-defect equilibrium. Equation (4) is a necessary and sufficient condition for this and identifies the critical group size relative to δ:

$$n^*(\delta) = \frac{b - b\delta}{c - b\delta} \text{ if } c > b\delta,$$

and ∞ otherwise. Q.E.D.

Proof of Proposition 2

Consider a low-ability member who obtains a discounted payoff

$$V_1 = \frac{b(1 - f)}{1 - \delta(1 - q)}$$

in the postulated equilibrium. Given the strategies of the other members, a unilateral switch by this member to a cooperative action in any period (followed by reversion to the postulated strategy) will lead to a discounted payoff of $\{b(1 - f + 1/n) - c_1 + \delta V_1\}$ instead of V_1, and the condition

$$c_1 > \frac{b}{n}\left[\frac{1 - \delta + q\delta + q\delta n(1 - f)}{1 - \delta + q\delta}\right]$$

$$\equiv \frac{b}{n} L_1$$

implies this will be unprofitable. Since in discounted dynamic programming strategies that are unimprovable in one step are optimal (Whittle 1982), this shows that low-ability members are playing an optimal strategy. Similarly a high-ability member is choosing an optimal response, as such a member obtains discounted payoff

$$V_2 = \frac{b(1 - f) - c_2}{1 - \delta}$$

in this equilibrium, and this exceeds what he would obtain by shirking once, that is, $b(1 - f - 1/n) + (1 - q)\delta V_2$, if

$$c_2 < \frac{b}{n}\left[\frac{1 - \delta + q\delta n(1 - f)}{1 - \delta + q\delta}\right]$$

$$\equiv \frac{b}{n} L_2$$

Q.E.D.

Proof of Proposition 3

As before, it suffices to show that in any normal period it is optimal for any member i to choose $\bar{p}$, given the strategies of other members. The stationary policy p_i yields i an ex ante utility of

$$V_i(p_i) = \frac{b\,(1 - 1/n)\,\bar{p} + (b/n - c_i)\,p_i}{1 - \delta p_i \bar{p}^{n-1} - (1 - p_i \bar{p}^{n-1})\,\delta^{T+1}}$$

since the probability of triggering off a defection phase is $(1 - p_i\bar{p}^{n-1})$. Let $S(p_i, \bar{p}, T)$ denote the derivative of V_i. Then as $\bar{p}$ goes to 1 and T to ∞, S converges pointwise to the function

$$\frac{\delta b\,(1 - 1/n) + (b/n - c_i)}{(1 - \delta p_i)^2},$$

which is positive for all p_i, by virtue of (4). Since the convergence is also uniform, there exists $\epsilon > 0$ and integer $\bar{T}$ such that whenever $0 < 1 - \bar{p} < \epsilon$ and $T > \bar{T}$, $\bar{p}$ maximizes V_i. Q.E.D.

Proof of Proposition 4

Given a cutoff level C and length T of noncooperative phases, the value function for member i in a cooperative period —given that all other members are pursuing the trigger strategy corresponding to (C, T)—is

$$V_i(p_i; C, T) = b\left(1 - \frac{1}{n}\right)\bar{p}$$
$$+ \left(\frac{b}{n} - c_i\right)p_i$$
$$+ \delta\,\{S_i(p_i, C)V_i(\bar{p}; C, T)$$
$$+ [1 - S_i(p_i, C)]\,\delta^T V_i(\bar{p}; C, T)\}$$

where $S_i(p_i, C)$ is the probability that collective output of the group—when $(n - 1)$ members are choosing $\bar{p}$, and the remaining member is choosing p_i— will achieve at least the cutoff level C. Clearly,

$$S_i(p_i, C) = \text{Prob}[\text{Bi}(n - 1, \bar{p}) \geqslant C]$$
$$+ p_i\,\text{Prob}[\text{Bi}(n - 1, \bar{p}) = C - 1]$$

if $\text{Bi}(n, \bar{p})$ denotes a binomial random variable corresponding to n trials with success probability $\bar{p}$, and

$$V_i(\bar{p}; C, T)$$
$$= \frac{(b - c_i)\,\bar{p}}{1 - \delta^{T+1} - \delta(1 - \delta^T)\,\text{Prob}[\text{Bi}(n, \bar{p}) \geqslant C]}$$

Hence for (C, T) to provide adequate effort incentives, it is necessary that $V_i(p_i; C, T)$ is increasing in p_i, that is (C, T) must satisfy the condition

$$\left(\frac{b}{n} - c_i\right) + \delta(1 - \delta^T)\,V_i(\bar{p}; C, T)$$
$$\cdot\,\text{Prob}[\text{Bi}(n - 1, \bar{p}) = C - 1] \geqslant 0,$$

and any such "feasible" pair gives rise to expected payoff $V_i(\bar{p}; C, T)$ for member with participation cost c_i. Part (a) of the result follows from the fact that $V_i(\bar{p}; C, T)$ is always decreasing in C, while $\text{Prob}[\text{Bi}(n - 1, \bar{p}) = C - 1]$ is decreasing in C as long as $C \geqslant n\bar{p} + 1$. Part (b) of the result follows from the fact that $\delta < 1$ implies that $(1 - \delta^T)\,V_i(\bar{p}; C, T)$ is increasing in T, while $V_i(\bar{p}; C, T)$ is decreasing in T. Q.E.D.

Proposition 5: Payoff Calculations and Proof

In this equilibrium a high-ability player attains ex ante expected utility (at the initial date) of

$$\frac{(b - c_2)\,\bar{p}}{1 - \delta[1 - q(1 - \bar{p})]} - \frac{b\bar{p}f}{1 - \delta[1 - q(1 - \bar{p})]}\cdot$$
$$\left[\frac{1 - \bar{p}}{1 - f\bar{p}} + \frac{\bar{p}(1 - f)\,\{1 - \delta[1 - q(1 - \bar{p})]\}}{(1 - fp)\{1 - d\delta[1 - q[1 - q(1 - \bar{p})]\}}\right] \tag{A3}$$

where

$$d \equiv 1 - q(1 - f\bar{p}).$$

At date 1 the expected number of low-ability members in the group at the end of date t is

$$E(m_t) = \frac{nf}{(1 - d)}$$
$$\{(1 - \bar{p})q - d^{t+1} + d^t[1 - q(1 - \bar{p})]\} \tag{A4}$$

Proof. That any low-abiliy member will choose $p_i = 0$ always follows from an argument similar to that used in proposition 2. For a high-ability member, it suffices to guarantee that $\bar{p}$ close enough to 1 implies that the expected number of low-ability members at any date and following any history does not exceed its value at date 1, that is, $n(1 - f)$. This is shown in Mookherjee (1984). So we simply need to verify (A3) and (A4).

Given m_{t-1} low-ability members at the end of $(t - 1)$, that is, $(n - m_{t-1})$ high-ability members at the beginning of t, the expected number of low-ability members at the end of t is

$$\begin{aligned} E(m_t|m_{t-1}) &= m_{t-1} - qm_{t-1} \\ &+ qfm_{t-1} + (n - m_{t-1})(1 - \bar{p})qf \\ &= dm_{t-1} + n(1 - \bar{p})qf, \end{aligned} \tag{A5}$$

since the expected number of low-ability members expelled at t is qm_{t-1} and of high-ability members is $(n - m_{t-1})(1 - \bar{p})q$, while fraction f of all incomers are low-ability. (A5) implies that

$$\begin{aligned} E(m_t|m_{t-k}) &= d^k m_{t-k} \\ &+ n(1 - \bar{p})qf(1 + d + \ldots + d^{k-1}) \end{aligned}$$

so

$$\begin{aligned} E(m_t) &= E(m_t|m_0 = nf) \\ &= \frac{nf}{1 - d}\{(1 - \bar{p})q - d_{t+1} \\ &+ d^t[1 - q(1 - \bar{p})]\}, \end{aligned}$$

which is (A4). The expected utility for a high-ability player at date t when viewed from date 1 is

$$\frac{b}{n}[n - E(m_{t-1}) - c_2]\,\bar{p},$$

so the long-run expected utility is

$$\sum_{t=1}^{\infty} \delta^{t-1}\{1 - q(1 - \bar{p})\}^{t-1} \cdot \left\{\frac{b}{n}[n - E(m_{t-1}) - c_2]\right\}\bar{p} \tag{A6}$$

since the effective discount must incorporate the probability $q(1 - \bar{p})$ of a high-ability player's being expelled at any date. Using (A4) reduces (A6) to (A3). Q.E.D.

Proof of Proposition 6

Using the result in part (b) of proposition 4, it suffices to show that for given $\bar{p}$ there does not exist in sufficiently large groups any cutoff level that provides members with effort incentives (in normal periods) in conjunction with infinitely long punishment phases. It is easily verified that low-ability players will cooperate in normal periods, given any cutoff C and length of punishment phases $T = \infty$, if and only if

$$\begin{aligned} &\text{Prob}[\text{Bi}(n - 1, \bar{p}) = C - 1] \\ &\geqslant \frac{c_1 - b/n}{\delta} \end{aligned}$$

Since the binomial distribution has the property that for given $\bar{p}$, the modal probability is of order $1/\sqrt{n}$ for large n, it follows for sufficiently large n that

$$\begin{aligned} &\text{Prob}[\text{Bi}(n - 1, \bar{p}) = C - 1] \\ &< \frac{c_1 - b}{\delta} \text{ for } \textit{all} \text{ possible values of } C, \end{aligned}$$

which establishes the result. Q.E.D.

Proposition 7 (Dual Regime): Payoff Calculations and Proof

In this equilibrium, member i obtains ex ante expected utility

$$\frac{\{b(1 - jq(1 - \bar{p}^j)) - c_i\}\,\bar{p} + \delta^{T+1} D_1}{1 - \delta\{1 - jq(1 - \bar{p}^j)\} - \delta^{T+2} D_2} \tag{A7}$$

where

$$D_1 \equiv jq(1 - \bar{p}^j)$$

$$\frac{(bjq\bar{p}^j - c_i)\,\bar{p}}{1 - \delta(1 - jq) - \delta^{T+1} jq(1 - \bar{p}^j)},$$

$$D_2 \equiv \frac{j^2 q^2 (1 - \bar{p}^j)\, \bar{p}^j}{1 - \delta(1 - jq) - \delta^{T+1} jq(1 - \bar{p}^j)}$$

Proof. Define an *abnormal* period to be any one of the T periods following an observation that the subgroup's performance was less than bj. Then future strategies of all members are unaffected by performance in such periods, making it optimal for each to choose $p_i = 0$ noncooperatively.

Define a *normal* period t as one where at $(t - 1)$ the subgroup was not punished by the center (i.e., when performance was last monitored it was bj, or it was never monitored before). Finally, define a *seminormal* period as one following a sequence of T abnormal periods since completion of which the subgroup has not been monitored. Clearly, seminormal periods arise with positive probability if $jq < 1$ and T is finite. In such periods the center will end the punishment only if performance is monitored and found adequate. Seminormal differ from normal periods in two ways. If performance is unobserved (a) the subgroup is punished that period and (b) the following period will be seminormal rather than normal. Nevertheless we show below that when (7) holds, if $\bar{p}$ is close to 1 and T is large enough, then in both normal and seminormal periods every member will chose $\bar{p}$, given that all others are doing likewise.

Let V_t^N and V_t^S denote the expected utility of i for the remainder of the game from t onwards if it happens to be normal and seminormal respectively. Then

$$\begin{aligned} V_t^S = {} & p_s^t [bjq\bar{p}^{j-1} - c_i] \\ & + \delta\{(1 - jq)V_{t+1}^S + jqp_s^t\bar{p}^{j-1}V_{t+1}^N \\ & + jq(1 - p_s^t\bar{p}^{j-1})\delta^T V_{t+T+1}^S\}. \qquad \text{(A8)} \end{aligned}$$

$$\begin{aligned} V_t^N = {} & [jqp_N^t\bar{p}^{j-1} + (1 - jq)]b\bar{p} - c_i p_N^t \\ & + \delta\{(jqp_N^t\bar{p}^{j-1} + 1 - jq)V_{t+1}^N \\ & + jq(1 - p_N^t\bar{p}^{j-1})\delta^T V_{t+T+1}^S\} \qquad \text{(A9)} \end{aligned}$$

where p_s^t and p_N^t are strategies chosen at t if it is seminormal and normal respectively. It suffices to show that postulated strategy $p_s^t = p_N^t = \bar{p}$ is *unimprovable*, for $\bar{p}$ close to 1 and T to ∞ (see Whittle [1982, chap. 28]). Under the postulated strategy V_t^N reduces to (A7) and V_t^S reduces to

$$\overline{V}^S = \frac{(b\bar{p}^j jq - c_i)\,\bar{p} + \delta\bar{p}^j jqD_3}{1 - \delta\{1 - jq[1 - \delta^T(1 - \bar{p}^j)]\} - \delta^{T+2}D_4} \qquad \text{(A10)}$$

where

$$D_3 \equiv \frac{b\bar{p}(1 - jq) + \bar{p}(bjq\bar{p}^j - c_i)}{1 - \delta[1 - jq(1 - \bar{p}^j)]},$$

$$D_4 \equiv \frac{j^2 q^2 \bar{p}^j (1 - \bar{p}^j)}{1 - \delta[1 - jq(1 - \bar{p}^j)]}$$

Letting $\hat{V}_t^S$ and $\hat{V}_t^N$ denote V_t^S and V_t^N obtained by replacing V_{t+1}^S, V_{t+1}^N and V_{t+T+1}^S by (A10), (A7), and (A10) respectively in (A8) and (A9), it can be checked that

$$\frac{\partial \hat{V}_t^S}{\partial p_s^t} = \frac{\partial \hat{V}_t^N}{\partial p_N^t},$$

and

$$\lim_{\substack{\bar{p}\to 1\\ T\to\infty}} \left(\frac{\partial \hat{V}^S}{\partial p_s^t}\right) = bjq - c_i + \frac{\delta jq}{1-\delta}[(bjq - c_i) + b(1 - jq)],$$

which is positive, given (7). Hence it is optimal to choose p_s^t and p_N^t equal to $\bar{p}_N^t$ equal to $\bar{p}$, given that this strategy is to be followed in the future, if $\bar{p}$ is sufficiently close to 1 and T to ∞. Q.E.D.

Proof of Proposition 8 (Completely Decentralized Federal Groups)

Define a period to be *normal* (N) if both local collective activity and subgroup contribution to the global organization were maximal in the previous period. Otherwise call a period *abnormal* (A). It is clear that given that all other members are following the postulated strategy, it is optimal for member i to choose $e_i = 0$ in an abnormal period.

Let (p_N, e_N) denote contributions in normal periods and p_A the contribution to the global organization in an abnormal period. Then

$$\begin{aligned} V^N(p_N, e_N) &= \frac{b}{n}[(n-1)\bar{p} + p_N] \\ &+ \frac{\beta}{j}(j - 1 + e_N) - c_i(e_N + p_N) \\ &+ \delta\bar{p}^{j-1} p_N V^N(p_N, e_N) \\ &+ \delta(1 - p_N\bar{p}^{j-1}) V^A(p_A), \text{ if } e_N = 1; \\ &= \frac{b}{n}[(n-1)\bar{p} + p_N] \\ &+ \frac{\beta}{j}(j - 1 + e_N) - c_i(e_N + p_N) \\ &+ \delta V^A(p_A), \quad \text{if } e_N < 1. \qquad \text{(A11)} \end{aligned}$$

$$\begin{aligned} V^A(p_A) &= \frac{b}{n}[(n-1)\bar{p} + p_A] - c_i p_A \\ &+ \delta\bar{p}^{j-1} p_A V^N(p_N, e_N) \\ &+ \delta(1 - p_A\bar{p}^{j-1}) V^A(p_A). \qquad \text{(A12)} \end{aligned}$$

It is obvious from (A11) that e_N is either 0 or 1. Also if e_N is 0 then the best p_N is also 0. Let $\tilde{V}^N$ and $\tilde{V}^A$ denote (A11) and (A12) when their right-hand sides have $V^N(p_N, e_N)$ and $V^A(p_A)$ replaced by $V^N(\bar{p}, 1)$ and $V^A(\bar{p})$ respectively. If then suffices to show that for $\bar{p}$ sufficiently close to 1, the following hold:

(a) $p_N = \bar{p}$ maximizes $\tilde{V}^N(p_N, 1)$.
(b) $\tilde{V}^N(\bar{p}, 1) > \tilde{V}^N(0, 0)$.
(c) $p_A = \bar{p}$ maximizes $\tilde{V}^A(p_A)$.

To prove (a) and (c), note that

$$\frac{\partial\tilde{V}^A(p)}{\partial p_A} = \frac{\partial\tilde{V}^N(p, 1)}{\partial p_N} = \left(\frac{b}{n} - c_i\right)$$

$$+ \delta\bar{p}^{j-1}[V^N(\bar{p}, 1) - V^A(\bar{p})],$$

which converges to

$$\left(\frac{b}{n} - c_i\right) + \delta(\beta - c_i) \qquad \text{(A13)}$$

as $\bar{p}$ converges to 1. (10) implies (A13) is positive. To prove (b), note that

$$\lim_{\bar{p}\to 1}[\tilde{V}^N(\bar{p}, 1) - \tilde{V}^N(0,0)] = \delta(\beta - c_i)$$

$$+ \left(\frac{b}{n} - c_i\right) + \left(\frac{\beta}{j} - c_i\right),$$

which is also positive by virtue of (10).
Q.E.D.

Notes

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1. Of course there are collective action problems where the situation is less stark, e.g., it may be worthwhile to contribute as long as a certain fraction of the group is doing so as well (Schelling 1978) or it may be worthwhile for a particularly large member to participate by himself if there are payoff asymmetries (Olson 1965, 45).

2. For some corroborating evidence in the case of the two-person game, see Murnighan and Roth (1983).

3. To be sure, dyadic ties may form very large networks: A deals with B, who reciprocates with C and so on. But in such circumstances each tie of reciprocity binds only two people.

4. In fact, it is a bit stronger than Nash, for the strategies ensure that the threat of retaliation is credible, whereas some Nash equilibria require noncredible threats. Equilibria supported by credible threats are known as perfect equilibria (Selten 1975). All the equilibria presented in this paper can be shown to be perfect in Selten's sense.

5. As noted earlier, discounting the future completely makes the game in effect a one-shot affair, and noncooperation becomes the only equilibrium strategy.

6. Specifically,

$$n^* = \frac{1 - \delta}{c/b - \delta} \text{ provided } \frac{c}{b} > \delta.$$

It is intuitively reasonable that the critical size increases in the benefit b and decreases in the cost c.

7. This idea can be generalized to cover n different levels of private costs (Mookherjee 1984), but because the qualitative insight is the same, we use the example of two types of members. For convenience we refer to the two types as low- and high-ability, but there are many other reasons why the costs of cooperation vary across members.

8. It may be possible to monitor and impose punishments on a decentralized basis (Axelrod 1986), particularly in small homogeneous groups. Because we are interested in cooperation in large groups, we assume that this authority has been delegated to a central office.

9. This assumption is made to ensure that the size of the group does not automatically dwindle as some members are expelled. It will be inessential in the present case where monitoring is perfect. It is, however, convenient in the context of imperfect monitoring, a case we shall soon consider. There it is also relatively inessential for large groups, provided the fraction of shirkers in the group is not large. At any rate it is a realistic assumption for professional groups that can adjust entrance requirements appropriately to replace expelled members. For other interest groups the expulsion of some members should not affect group size materially in the long run, as new generations of young members join the group over time.

10. Alternatively, this uncertainty may be interpreted as the possibility that decision makers are not always perfectly rational—a fraction of the time each member may make the mistake of choosing a suboptimal alternative. In the game-theoretical context this significant departure from the classical assumption of complete rationality was pioneered by Selten (1975).

11. A previous version of this paper (Bendor and Mookherjee 1985) also considers the case of perfectly correlated uncertainty. Our basic argument will, however, extend to imperfect correlation insofar as one can view such a case as a mixture of perfectly correlated and independent uncertainties.

12. This assumption can also be weakened, i.e., free riding could with a small probability yield a positive contribution. Because this modification complicates the calculations while leaving our essential results unchanged, we use the simpler assumption noted in the text.

13. We are excluding from consideration trigger strategy equilibria that induce only high-ability members to work in cooperative periods, with low-ability members shirking always. Such an arrangement would be dominated by the centralized structure since it would have the same amount of free riding and would be less stable as well.

14. The chance of breakdown is given by the probability that a binomial random variable with n trials and $\bar{p}$ probability of success falls below the cutoff C_n.

15. Note that this uncertainty may also capture mistakes that the monitor can make in assessing the contribution of any member, so our model does not depend on the assumption of a perfect monitor.

16. To calculate the results of Table 1, we set $b = 500$, $c_1 = 100$, $c_2 = 20$, and $\delta = 0.9$. We then used a brute force algorithm to find the values of C_n that would support an equilibrium for $T_n = \infty$. Next, using the second part of proposition 4, we searched for the smallest T_n that would sustain a given C_n. Finally, for a given n, we selected the $\{C_n, T_n\}$ pair that maximized expected payoffs to the members. Though proposition 4 does not include this property, it turned out that the lowest sustainable C_n was always the best.

17. Note that every time the shortfall increases, the punishment period T_n rises as well. This more severe threat compensates for the less demanding performance standard in order to provide the required participation incentives.

18. This proposition can easily be extended to the case of continuous output. For example, suppose that an individual's contribution is a function of effort plus a normally distributed disturbance of mean zero. As before, disturbances are independent over time and across members. Since collective output is the sum of individual contributions, it, too, will be normally distributed, with a variance equal to the sum of the variances of the individual perturbations. Hence once again the output distribution will flatten out in large groups, and for sufficiently large n, the chance that any member will be pivotal will fall arbitrarily close to zero. Thus, although in our model collective performance is discrete, the validity of proposition 6 does not rest on this assumption.

19. At first glance proposition 6 may seem to contradict proposition 3's claim about the existence of a cooperative equilibrium in a trigger strategy regime. However, proposition 3 asserted that for a *fixed n*, a trigger strategy equilibrium existed provided the disturbance $(1 - \bar{p})$ was sufficiently small. Proposition 6, on the other hand, holds the disturbance constant and varies n.

20. This assumption of linear increases in the monitoring probability is convenient for computing the numerical examples. The qualitative pattern of results, however, really depends on the much weaker assumption that the probability a chapter will be monitored increases monotonically with its size.

21. It should be emphasized that expected payoffs in both the centralized regime and the federated regime with fixed chapter size are independent of the

size of the overall organization. This is in contrast with the unfederated decentralized structure, which tends to perform more poorly for larger organizations.

22. Of course industrial sociologists have long recognized that informal social pressure is often brought to bear on those who do not pull their weight in instrumental activities. Hardin's contribution is to set this in the context of the n-person repeated play of the prisoner's dilemma.

23. The assumption that the local game is a prisoner's dilemma may not be satisfied if the interactions are purely social in nature. Yet the local games may still help regulate the global one. For example, suppose each member of a chapter faces a binary choice of being friendly $\{f\}$ or unfriendly $\{u\}$ to other members of the chapter. Because most people like to reciprocate social affect, there will be *two* Nash equilibria in the one-shot game, a benign one $\{f,f\}$ and an unpleasant one $\{u,u\}$. The existence of two equilibria indicates this is not a prisoner's dilemma; nevertheless, these equilibria provide opportunities for punishing peers who shirk in the global game. Suppose n-1 players use the following twin strategies: (1) always contribute to the global cause; (2) locally, be friendly to everyone who works hard for the global benefit and ostracize free riders for T periods. (See Dawes, McTavish, and Shaklee [1977, 7] for some fascinating data on how experimental subjects spontaneously create such strategies.) For sufficiently big T, the n^{th} player's best response may be to contribute to the global good and to uphold the local social code. Of course, as usual this would be only one perfect equilibrium. There probably are collusive equilibria wherein the local social code involves defecting from the global concern and punishing nondefection.

24. One could generalize proposition 8 by allowing the monitoring of the local activities to be imperfect and by permitting the costs of local participation to differ from those of contributions to the global good, as in (8). The equilibrium would remain essentially unchanged.

25. For some evidence on how easily subgroups develop out-group bias, see Kramer and Brewer (1984) and the references cited therein.

26. The list of all relevant citations would be extremely long. See, for example, Crozier (1964); Dalton (1959); Gouldner (1954, 1960); Heclo (1977); Matthews (1960); Miller and Form (1964, 280–81); Moore (1951, 282–90); Roethlisberger and Dickson (1939, 525); Sapolsky (1972, 122); Selznick (1949, 251–52); Shils and Janowitz (1948); Simon, Smithburg, and Thompson (1950, 87); Stouffer, Lumsdaine, Lumsdaine, Williams, Jr., Smith, Janis, Star, Cottrell, Jr. (1949); Zaleznik (1956, 1965); and so on.

27. This regularity is so pervasive that the ambitions of early organizational reformers such as Frederick Taylor to design organizations from top to bottom, to create systems unmarked by any informal structure, are now recognized as utterly utopian.

28. See Truman (1951, 112–29) for a description of the hierarchic structure of several important interest groups in the United States. More recently, see Knoke and Wood (1981, 37–41).

29. Note that in our model the benefits of localizing shocks can be obtained by creating only one level of subunits. Deeper nesting is unnecessary. Consequently, other considerations are required to explain the empirical prevalence of tall hierarchies.

30. Axelrod's simulations of the evolution of cooperation (1984) strongly suggest that computer simulations could be a very valuable tool for exploring dynamics.

31. Though their analyses were not game-theoretical, functionalists in sociology and anthropology have similarly argued that explaining the origins of institutions was a problem for historians rather than social scientists. See Stinchcombe (1968, 105) for a lucid explication of this view.

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INFLUENCES ON EXECUTIVE AND CONGRESSIONAL BUDGETARY PRIORITIES, 1955–1981

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A reduced form equation system is used to analyze the influence of economic, political, and institutional influences on the budgetary priorities of the executive branch and Congress during fiscal 1955–81. Three related issues are considered: the extent to which political and macroeconomic factors affect priorities; the degree of interdependence among the components of the federal budget and between spending and revenues; and differences between the executive branch and Congress with respect to these issues. Both types of interdependence are present within both executive branch and congressional budgeting, although this interdependence is stronger within the executive branch. The influence of economic conditions on budgetary outcomes is strong but varies considerably across spending categories. There is no evidence of a political business cycle. Political variables exert a modest influence on the budgetary outcomes examined; differences between Democratic and Republican budgetary policies, as well as differences in the budgetary priorities of different presidential administrations, are small by comparison with the differences between executive and congressional policies.

We analyze and compare the budgetary priorities of the U.S. Congress and executive branch during a substantial portion of the postwar period in order to better understand the effects of the condition of the economy on these priorities. We measure budgetary priorities as the shares of gross national product accounted for by the major components of federal spending—defense, domestic controllable, and domestic uncontrollable outlays—in the budgets emerging from the congressional and executive branch policy processes. We address three related issues: the extent to which political and macroeconomic factors influence budgetary priorities; the extent of interdependence among budgetary components and between spending and revenue; and the differences in these traits between the executive and congressional budgetary processes.

An important feature of the analytic framework we develop is the integration of several groups of influences on federal spending that have been considered separately in previous work. While the annual federal budget represents an important statement of policy in many areas, extending from macroeconomic policy objectives to program-specific spending priorities, the empirical literature on budgeting has rarely considered the full set of potential influences on

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budgetary outcomes. Such fragmentation means that budgetary models and theories, each of which incorporates a subset of the multiplicity of goals embedded in the annual federal budget, often yield inconsistent or mutually contradictory results—models of "political economics," for example, reach conclusions that differ substantially from those of "incrementalists."

Our model allows for an empirical assessment of the relative influence on budgetary outcomes of fiscal pressures, political variables, economic fluctuations, program-specific spending pressures, and spending pressures in other programs. The nature of tradeoffs among competing policy objectives is estimated statistically, rather than being assumed. Our analysis of budgetary outcomes is both disaggregated, separately examining different components of the budget, and comprehensive, since it includes most of the components of federal spending. The model also distinguishes between the budgetary decisions of the executive branch and Congress, in contrast to much of the previous literature.

Fragmentation in the Budgetary Literature

Inasmuch as the federal budget affects policy in many different areas, scholars have approached the analysis of budgets from varied intellectual perspectives. The resulting heterogeneity means that individual studies of budgetary processes and outcomes have been unable to build on prior studies, while factors deemed central in some parts of the literature have been ignored in other work. Tests among competing theories of budgeting have been hampered by this heterogeneity.

One major strand of the empirical literature has considered the sensitivity of macroeconomic policies to political influences.[1] While the literature in political economics has been provocative, it has neglected several important aspects of the relationship between political variables and economic outcomes. Political economy studies have rarely specified the policy instruments employed to manipulate macroeconomic outcomes. Observed economic outcomes also have been widely assumed to be intended outcomes. Rather than proposing a detailed theory of the formulation of policy goals and the implementation of policies to realize these goals, scholars have endowed policy makers with remarkable powers of forecasting and fiscal "fine tuning."[2]

In addition to ignoring the link between outcomes and intentions, much of the literature on political economics has aggregated executive and legislative policy processes. Policy outcomes have been described "as if" they resulted from the actions of a single actor, usually assumed in the U.S. context to be the president.[3] Such aggregation of executive and congressional branches ignores the interplay of political and institutional factors that determine budgetary outcomes. A number of works, for example, Niskanen (1971, 1975), Fiorina (1978), Shepsle (1983), and Kamlet and Mowery (1983, 1985a),[4] have suggested that the institutional structure of Congress limits interdependence in budgetary decisions and supports spending growth, implicitly contrasting congressional with executive budgetary behavior. Nonetheless, the empirical implications of these theoretical differences between executive and congressional budgetary incentives have rarely been examined.

If the political economy literature has neglected institutional structure and the policy process, the literature on the bureaucratic politics of budgeting, which includes most of the incrementalist interpretations of budgeting, has largely ignored traditional political influences. Within this literature, bureaucratic policy processes are governed by organizational procedures and decision rules and display little sensitivity to higher-level political influences (Padgett 1981).

The tradeoffs among competing policy objectives that are inherent in budgetary decision making also receive little attention in the empirical literature. Some studies, for example, restrict their focus to total spending or the size of the deficit (e.g., Hicks 1984; Lowery and Berry 1983; and Pack 1985). These studies cannot provide an explanation of spending patterns in specific program areas, however, nor are they able to incorporate the influence of programmatic priorities on total spending. The omission is serious, because macroeconomic stabilization is but one of a number of influences on budgetary outcomes. Multiple policy objectives are pursued simultaneously through the budget; commitments to individual programs, for example, may restrict the freedom of congressional or executive branch policy makers to pursue economic policy goals. The few works incorporating tradeoffs among competing policy objectives (e.g., Auten, Bozeman, and Cline 1984; Domke, Eichenberg, and Kelleher 1983; Fischer and Crecine 1981; and Russett 1982) typically have assumed the nature of such tradeoffs (e.g., assuming that the budgetary process is purely top-down, with fiscal policy goals dominating programmatic objectives), rather than estimating or testing such assumptions.

Other studies, including most incrementalist and "arms race" models of defense spending, have assumed that the budgets of individual programs are independent.[5] This research presupposes the budgetary process to be purely bottom-up; each agency's budget is independent of spending pressures within other expenditure categories, fiscal policy priorities, or high-level political and economic considerations.

The analytic framework we propose acknowledges the wide range of objectives that may influence budgetary outcomes and allows their relative importance to be determined empirically, rather than by assumption. This analysis extends Fischer and Kamlet (1984) in several ways. We examine congressional as well as executive budgeting, consider the importance of political and economic influences on budgetary outcomes in greater detail, and analyze uncontrollable and controllable domestic programs as separate budgetary components. The model provides a framework within which both the Competing Aspirations Levels Model (CALM) framework of Fischer and Kamlet (1984) and the broader literature on incrementalism and political economy can be situated.

Modelling Budgetary Tradeoffs

Interdependence in budgetary decision making refers to the extent to which budgetary outcomes in specific categories are sensitive to (1) pressures for growth in other programs and (2) fiscal policy pressures, as these latter influences are reflected in projected revenues and implied surplus or deficit levels. Consider the following general specification:

$$D = f_d(X_d, X_c, X_u, X_T, C, U, T) \qquad (1a)$$
$$C = f_c(X_d, X_c, X_u, X_T, D, U, T) \qquad (b)$$
$$U = f_u(X_d, X_c, X_u, X_T, D, C, T) \qquad (c)$$
$$T = f_T(X_d, X_c, X_u, X_T, D, C, U) \qquad (d)$$
$$D + C + U + I = T \qquad (e)$$

Within this system, D represents defense outlays for a given year, C is the level of controllable nondefense outlays, U is the level of uncontrollable nondefense outlays (excluding interest payments), I is the level of interest payments, and T is the level of total outlays. The influences on the level of spending in each category include the amounts spent on other categories. Exogenous, program-specific influences are captured in the X_d, X_c, and X_u terms, respectively affecting defense, controllable domestic, and uncontrollable spending. For example, last year's defense budget, the perceived level of the Soviet military threat, and the level of armed conflict all may affect defense priorities.

This model also incorporates macroeconomic objectives and influences on total spending. These influences are captured in the X_T term, which is a vector of variables measuring various dimensions of macroeconomic goals and the economic environment. The X_T term includes the level of projected revenues and the state of the economy, both of which are hypothesized to affect fiscal policy objectives and therefore the desired level of total outlays. X_T also includes political influences, such as the party composition of Congress and the political affiliation of the president. These aggregate political and economic influences may be included as well in one or more of the X_c, X_d, or X_u vectors, reflecting the fact that they directly affect spending in a specific category. Changes in the level of unemployment, for example, which are included in X_T, may affect uncontrollable spending directly, through the operation of such automatic stabilizers as unemployment payments and thus are also included in the X_u vector of variables.

Equations (1a)–(1d) are structural equations modelling the influence of various factors on defense outlays, controllable domestic outlays, uncontrollable outlays, and total outlays, respectively. Equation (1e) is simply an accounting identity, indicating that total expenditures are by definition the sum of the individual expenditure components. Any model of budgetary decisions must be consistent with this accounting constraint. Equation system (1) is a system of simultaneous, structural equations. Its reduced form representation is

$$D = h_d(X_d, X_c, X_u, X_T) \qquad (2a)$$

$$C = h_c(X_d, X_c, X_u, X_T) \qquad (b)$$

$$U = h_u(X_d, X_c, X_u, X_T) \qquad (c)$$

$$T = h_T(X_d, X_c, X_u, X_T) \qquad (d)$$

$$T = D + C + U + I \qquad (e)$$

The system represented by equations (2a)–(2e) can incorporate both program-specific and aggregate influences on budgetary priorities while at the same time testing for interdependence. The hypothesis that spending levels within different budgetary categories are independent of one another assumes that equations (2a)–(2e) are identical to the following system:

$$D = g_d(X_d) \qquad (3a)$$

$$U = g_u(X_u) \qquad (b)$$

$$C = g_c(X_c) \qquad (c)$$

$$T = D + U + C + I \qquad (d)$$

In this formulation, defense budgets are formulated solely with reference to defense considerations; no influence is exerted by nondefense or fiscal priorities. Similarly, uncontrollable domestic and controllable domestic budgetary decisions are each made solely on the basis of parochial considerations and are not influenced by defense or fiscal priorities. Incrementalism is one example of such a model. In the pure incrementalist specification, g_d, g_u, and g_c are linear and X_d, X_c, and X_u are simply prior-year budget allowances. In any independent process model, fiscal policy priorities (e.g., revenues) exert no influence on budgetary allocations. Total expenditures are the sum of $g_d(X_d) + g_u(X_u) + g_c(X_c) + I$.

The hypothesis of budgetary interdependence may be tested by examining the role of nonparochial variables in equations (2a)–(2c). Nonparochial influences fall into two broad classes. One class is program-specific variables that affect spending in other programs. For example, interdependence between other budgetary categories and domestic controllable spending is present if X_d or X_u exerts a significant influence in equation (2b). Yet another form of budgetary interdependence hypothesizes that fiscal policy goals constrain total spending and thus affect spending on individual programs—indeed, this top-down fiscal constraint underpins the interdependence among spending categories. A number of researchers have argued on the basis of statistical and archival evidence that budgetary interdependence within the executive branch is enforced by the presence of a top-down spending constraint due to the salience of deficits or surpluses for economic stabilization (e.g., Fischer and Kamlet 1984; Mowery, Kamlet, and Crecine 1980). The effect of these nonparochial, aggregate influences on budgetary outcomes may be tested by examining the impact of the variables in the X_T vector.

In converting the functional restrictions of equation system (2) into a form amenable to statistical analysis, we assume that equation system (1)—and therefore equation system (2)—is linear. For the hypothesis of independence in budgetary decision making to be supported, the parameters in equation system (2) for variables representing parochial influences on spending in one category must be equal to zero in the equations for other budgetary categories.

In addition to examining interdependence in budgetary decision making, equation system (2) also allows for tests of a number of the hypotheses associated with the political economy literature. For instance, the influence on budgetary outcomes of political variables, such as the party of the president or the proximity of elections, can be tested by incorporating these variables in the X_T, X_u, X_c, and/or X_d vectors. The influence of these variables on budgetary outcomes can be assessed within a framework that is less likely to confound their influence with that of program-specific spending pressures or other influences on fiscal and budgetary policy (e.g., the state of the economy).

Data Sources, Variable Construction, and Specification Issues

Dependent Variables

The variables employed in our regression analyses are listed and defined in the Appendix. We examine three dependent variables: controllable domestic outlays, uncontrollable domestic outlays, and defense outlays. Defense outlays refer exclusively to outlays by the Department of Defense. Uncontrollables include the major domestic entitlement programs and exclude interest payments (which are responsive to factors other than those affecting the remaining uncontrollable programs). Controllable domestic outlays are computed as the difference between total spending and the sum of defense, uncontrollable, and interest outlays. Each dependent variable is computed for the president's budget, the budget following congressional action, and the final estimates of outlays after the end of the fiscal year. Outlays, rather than budget authority, were utilized because fiscal policy pressures (reflected in the budgetary surplus or deficit) typically operate through outlays.[6] All expenditure and revenue variables are measured as a share of GNP.

Predetermined Variables

The model incorporates as predetermined variables both independent and lagged endogenous variables. The independent variables consist of projected revenues, inflation, unemployment, the level of armed conflict in which U.S. forces are engaged, the perceived Soviet threat, the number of northern House Democrats, and dummy variables indicating the proximity of national elections, the administration in office, and the party of the president. These variables are defined and discussed in the Appendix.

All of our specifications include lagged endogenous variables. Most models of budgetary decision making ascribe great importance to prior-year expenditure allowances. Incrementalism, for instance, views prior-year budgetary allowances as a "base" and argues that policymakers focus on slight increments or decrements from this base. Similarly, prior-year budgetary allowances play a central role in models of spending pressure, where these growth pressures are assumed to be approximately proportionate to past expenditure levels. The use of lagged endogenous variables in our regressions raises problems of specification and estimation.[7] Despite these difficulties, lagged endogenous variables are included in these equations because of the substantive importance of prior-year spending levels in most theories of budgetary decision making. In addition, the inclusion of lagged values of the dependent variables helps to control for lags in the adjustment of spending to exogenous influences.[8]

Specification Issues

Several specification issues arise in the empirical application of our analytic framework. One issue concerns the formulation of the variables measuring unemployment and the level of armed conflict—should they be represented in first difference form or as absolute magnitudes? Either formulation of these variables can be defended, depending on assumptions concerning the nature of policy maker perceptions and responses. The formulation of these explanatory variables is based on a priori hypotheses and empirical investigation and is discussed in greater detail in Kamlet and Mowery (1985b). The Appendix briefly describes the formulation of each variable for the benchmark specifications. In general, the estimated values of the parameters for other variables in our analysis were not significantly affected by the substitution of alternative formulations of these variables.

A second issue concerns the nature of the lags used for the U.S.-USSR tension, unemployment, war, and inflation variables. These lag structures are also based on a combination of theory and experimentation. The measure of Soviet military tensions employed in the analysis of spending for fiscal year t is that prevailing during calendar year t-2, that is, the perceived Soviet threat for the year during which the executive branch formulates fiscal year t's budget. The choice of lags for the economic variables incorporated in the equation for the president's budget was straightforward because forecasts of these variables employed in the formulation of the president's budget generally were available. In the absence of comparable forecasts for congressional budgeting, we used for fiscal year t the inflation and unemployment rates for the second and third quarters of calendar year t-1. This formulation assumes a purely extrapolative model of inflationary and unemployment expectations, that is, we assumed that current trends are expected by congressional actors to continue into the future.[9] Finally, we chose the level of armed conflict that prevails during calendar year t-1 as the basis for forming expectations about the level of armed conflict during fiscal year t. This assumption is discussed in greater detail in Fischer and Kamlet (1984). As was the case above, the coefficients for the other variables in our analysis were not substantively affected by the lags chosen for these variables.

Comparing Executive and Congressional Budgetary Behavior

In discussing our empirical results we take the figures proposed in the president's budget as a measure of presidential priorities and the congressional modifications of these figures as a measure of congressional budgetary priorities. This interpretation does not rule out presidential involvement in the congressional budget process so long as presidential policy goals remain consistent with those of the presidential budget document. If changes in presidential goals following the release of the budget occur in a nonrandom fashion across spending programs and years, however, this interpretation of the presidential and congressional budgets may be incorrect. For instance, misrepresentation or biased estimation of economic and demographic assumptions or other program data could allow for the release of a fiscally prudent president's budget that slides into a growing pool of red ink during congressional consideration. Attributing responsibility to Congress for all alterations in the president's spending requests under these circumstances clearly is unrealistic. Systematic bias in presidential short-run forecasting, however, seems modest (see Kamlet, Mowery, and Su 1986). Executive branch economic forecasts also must be consistent with revenue estimates in the president's budget. Much of the impact of biased forecasts is controlled for by the inclusion of contemporaneous revenue estimates as independent variables in both congressional and presidential regressions.

Apart from biases in forecasts and cost estimates, the president's budget may also reflect strategic positioning relative to anticipated congressional sentiment. However, the nature of any bias that this behavior imparts to the presidential budget is unclear (see Kiewiet and McCubbins 1985a, 1985b), nor are there strong reasons to believe that such biases will not offset one another across programs and years. Still, even if systematic gaming is present in the president's budget, the analyses of executive budgetary priorities can be interpreted as reflecting the budgetary priorities the executive branch wishes to convey to the public, rather than the president's true budgetary priorities. This interpretation of the president's budget as a political document is not new, but neither is it uninteresting.

Estimation Results

Tables 1–3 contain the results of specifications for domestic uncontrollable, defense, and domestic controllable spending for the executive branch and Congress jointly (Table 1), the president's budget (Table 2), and congressional budgetary actions on presidential budget requests (Table 3). These specifications test for the existence and extent of budgetary interdependence, the responsiveness of budgetary actions to economic fluctuations, and the impact of two political influences—the proximity of presidential and congressional elections and the party composition of Congress—on budgetary outcomes.

If budgetary interdependence is enforced by the presence of a top-down spending constraint, the regressions analyzing executive branch spending for the three budgetary components should yield a positive coefficient for the revenues term and negative coefficients for the variables associated with parochial spending pressures for other spending categories. Additionally, based on the atomistic nature of congressional decision making and the difficulties of implementing top-down control in a decentralized setting, we hypothesize that interdependence in executive branch budgetary outcomes is greater than in congressional budgetary outcomes.

Our priors are more diffuse concerning the economic and political variables. The economic variables measure the extent to which economic stabilization objectives influence the target spending total. They also capture the influence of macroeconomic conditions on program-specific spending outcomes. These top-down and bottom-up channels for spending pressure may be mutually reinforcing or offsetting. For instance, higher unemployment may increase the spending target total as a means of offsetting a recession. This top-down expansionary influence on uncontrollable spending will be reinforced by the operation of automatic stabilizers that affect uncontrollable spending, as expenditures on such programs as unemployment insurance grow. The impact of increased inflation, however, on top-down and program-specific spending pressures may be quite different. Increased inflation will reduce targets for total spending, as a countercyclical response. This decrease in the target total will exercise a depressive top-down influence on uncontrollable spending. Simultaneously, the impact of inflation on indexed uncontrollable programs will increase spending pressures associated with these programs.

Hypotheses concerning the influence of political variables center on the empirical validity of the "political business cycle," as well as the importance of political ideology in affecting both public spending and macroeconomic policy objectives. A number of authors, including Nordhaus (1975), MacRae (1977), Tufte (1978), Frey and Schneider (1978a, 1978b, 1979), and Hicks (1984), report empirical results that suggest that the economy is manipulated so as to reach a peak during election years.[10] Because the disbursement of federal funds takes place primarily during the calendar year following that during which the budget is considered by Congress, the year immediately prior to the election year (i.e., the third year following a presidential election) should acquire a positive and significant coefficient if presidential elections induce greater spending. The findings of Hibbs (1977) and others that political ideology significantly influences spending behavior and macroeconomic policy leads to the hypothesis that Republican control of Congress or the White House is associated with lower spending, ceteris paribus, with the possible exception of defense outlays.

The results in Table 1 concern budgetary outcomes at the conclusion of congressional action, including prior-year spending levels among the predetermined variables. These results therefore incorporate the joint impact of executive and congressional budgetary processes on budgetary outcomes. Defense spending (column 1) is heavily influenced by parochial defense considerations but displays substantial interdependence. Defense spending is negatively influenced by uncontrollable spending pressures and positively by revenues. A one-dollar increase (decrease) in revenues leads to an estimated 30¢ increase (decrease) in defense spending. An increase (decrease) in uncontrollable spending pressures, as reflected in a one-dollar increase (decrease) in prior-year uncontrollable spending, leads to an estimated 18¢ decrease (increase) in defense spending.

Interdependence within the uncontrollable and controllable spending categories is weaker. For uncontrollable programs, the coefficients for two of the three parochial defense variables, the parochial controllable spending variable, and the revenues variable have signs that are consistent with interdependence, although none is statistically significant. For controllable domestic programs, one of the five relevant coefficients, that for the perceived Soviet threat measure, has a sign consistent with interdependence and is significant. None of the other four coefficients, however, is statistically significant or the sign hypothesized by interdependence. The only other significant coefficient among the spending pressure variables in the domestic controllables regression is that for prior-year controllable expenditures, a result consistent with a purely bottom-up or incrementalist decision process.

The results presented in the last column of Table 1 employ the sum of the three expenditure components as the dependent variable. This specification allows for an assessment of the net impact of the various budgetary, political, and fiscal policy variables on total spending. Total expenditures are positively influenced by parochial spending pressure variables for all three expenditure categories, as well as the level of available revenues. In other words, total federal spending is determined by a combination of bottom-up (i.e., program-specific) and top-down (i.e., revenue-based) pressures.[11]

The coefficients for the variables other than the lagged endogenous ones in Table 1 are also of interest. Our measure of the level of armed conflict is significant in the defense spending specification and in the equation for total spending. The perceived Soviet threat variable assumes the

Table 1. Benchmark Specification, Congressional Outcomes Regressed on Prior-Year Variables

	Dependent Variable						
Independent Variables	Congressional Defense Outlays		Congressional Uncontrollable Outlays		Congressional Controllable Outlays	Congressional Total Outlays	
Constant term	−.02	−.02	.01	.01	.04	.02	.02
	(.02)	(.02)	(.02)	(.02)	(.03)	(.03)	(.03)
Prior-year actual uncontrollable outlays	−.18[a]	−.18[a]	.79[b]	.80[b]	.19	.80[b]	.79[b]
	(.10)	(.09)	(.10)	(.08)	(.15)	(.16)	(.15)
Prior-year actual defense outlays	.54[b]	.51[b]	−.15	−.15	.09	.47[b]	.45[a]
	(.14)	(.11)	(.15)	(.12)	(.21)	(.23)	(.19)
Level of armed conflict	.93[b]	.90[b]	−.27	−.26	.43	1.10[a]	1.10[a]
	(.32)	(.30)	(.34)	(.29)	(.48)	(.52)	(.49)
Perceived Soviet threat measure	.88	.99[b]	.38	.40	−1.78[a]	−.52	−.54
	(.66)	(.35)	(.68)	(.59)	(.97)	(1.10)	(.98)
Prior-year actual controllable outlays	.02		−.17	−.17	.52[a]	.37	.36
	(.14)		(.15)	(.13)	(.21)	(.23)	(.21)
Congressional revenues	.30[a]	.32[b]	.10	.08	−.05	.35[a]	.37[a]
	(.11)	(.06)	(.12)	(.09)	(.17)	(.18)	(.15)
Congressional unemployment measure	.27[a]	.26[b]	.06	.05	.21	.54[b]	.56[b]
	(.10)	(.09)	(.11)	(.09)	(.15)	(.16)	(.15)
Congressional inflation measure	−.12	−.13[a]	.09	.10[a]	−.13	−.16	−.16[a]
	(.07)	(.06)	(.07)	(.06)	(.10)	(.11)	(.09)
Second year after presidential election	−.17		.04		.16	−.03	
	(.20)		(.21)		(.30)	(.32)	
Third year after presidential election	−.09		.06		−.06	−.09	
	(.20)		(.21)		(.30)	(.33)	
Fourth year after presidential election	−.40[a]	−.32[a]	−.05		−.33	−.78[a]	−.77[b]
	(.19)	(.14)	(.20)		(.29)	(.31)	(.23)
Northern House Democrats	.59	.56	.17		.25	1.02	1.01
	(.44)	(.41)	(.47)		(.66)	(.71)	(.67)
R^2	.97	.97	.99	.99	.58	.89	.89
$\bar{R}^2$	.95	.96	.98	.98	.22	.79	.82
Durbin-Watson	2.41	2.39	1.82	1.79	2.29	2.45	2.44

Note: Standard errors in parentheses.

[a] Significant at a 95% confidence level, one-tailed test.

[b] Significant at a 99% confidence level, one-tailed test.

expected sign in the defense and domestic spending equations. Its significance in the defense outlays equation is in some contrast with work of other scholars who have examined the statistical link between Soviet military behavior and the U.S. military budget.[12]

Increases in unemployment (controlling for revenues) have a stimulative effect on all three components of total spending, and a positive and significant effect on total spending. Each category of expenditures, as well as total spending, responds countercyclically to unemployment fluctuations. Moreover, the significant positive coefficient for unemployment in the defense spending equation suggests that the response of spending to unemployment includes both discretionary and automatic components. Higher inflation (controlling for revenues) is associated with lower spending for defense and controllable domestic programs, also consistent with a countercyclical spending posture. However, increases in inflation are associated with *increases* in spending for uncontrollable programs, which partly offset the countercyclical response to inflation of the other budgetary components.[13] The procyclical impact of inflation on uncontrollable spending may reflect the operation of benefit indexing as well as discretionary benefit increases for uncontrollable programs during the inflationary 1970s.

Controlling for revenues, a 3.4% increase in inflation is required to decrease federal spending by an amount equal to the spending increase associated with a 1% decrease in unemployment.[14] Combining these results with those of Kamlet and Mowery (1986) concerning the impact of inflation and unemployment on revenues allows for an assessment of the total effect of inflation and unemployment fluctuations on spending (i.e., including the impact of economic fluctuations on spending that operates through revenue fluctuations). Accounting for the impact of economic fluctuations on revenues increases the disparity in the responses of federal spending to inflation and unemployment, with a 4.2% increase/decrease in inflation required to invoke the same total spending response as a 1% decrease/increase in unemployment. This seemingly asymmetric response of federal politicians to inflation and unemployment is consistent with arguments advanced by Buchanan and Wagner (1977) and others.

Is a political business cycle operating within this model? The coefficient for the dummy variable representing the third year after a presidential election generally has the wrong sign to support a political business cycle interpretation of budgetary behavior, and the only significant election proximity variable is that for the fourth year after a presidential election. The coefficient for this variable is negative in all the specifications and statistically significant in the defense and total spending equations. Rather than pursuing expansionary fiscal policies in the budget that takes effect during a presidential election year, budgetary outlays are held down for the budget debated during the campaign, which takes effect the following year. This effect seems strongest for defense spending. The results do not allow rejection of the hypothesis that political business cycles may be induced by manipulating the disbursement of expenditures *within* an election year, for instance, greater spending in the six months prior to the election. Election year effects on spending may also be more pronounced for individual programs included in one of the aggregate spending categories examined here.[15] Nonetheless, these results cast considerable doubt on the relevance of the electoral business cycle to this instrument of federal economic policy.

Political ideology, as measured by the number of northern House Democrats, also exerts a modest influence on budgetary outcomes. The northern House Democrat coefficient is positive in all the regressions in Table 1, but the coefficients are small and statistically insignificant. Other measures of congressional ideology —total House Democrats, Senate Democrats, total Democrats in the House and Senate, and a measure of congressional ideology derived by Poole and Daniels (1985) also yielded small and statistically insignificant coefficients. These findings must be interpreted cautiously, however, in view of the high level of aggregation of our data.[16]

The results of estimating a modified benchmark specification for presidential budget requests are shown in Table 2.[17] Defense spending again displays strong interdependence. In addition to being positively influenced by the parochial defense variables, the defense budget is negatively influenced by uncontrollable expenditure pressures and positively by available revenues, results that are similar to those in Table 1. In contrast to the results in Table 1, the results in Table 2 for uncontrollable spending in the executive branch suggest considerable inter-

Table 2. Presidential Budget Figures Regressed on Prior-Year Variables

	Dependent Variable				
Independent Variables	Defense Outlays	Uncontrollable Outlays	Controllable Outlays	Controllable Outlays	Total Outlays
Constant term	.00 (.01)	-.00 (.01)	.02 (.02)	.03 (.02)	.02[a] (.01)
Prior-year actual uncontrollable outlays	-.13[a] (.06)	.81[b] (.08)	.07 (.13)	.06 (.11)	.75[b] (.09)
Prior-year actual defense outlays	.60[b] (.10)	-.16 (.13)	.13 (.20)	.04 (.18)	.57[b] (.14)
Level of armed conflict	.85[b] (.22)	-.52[a] (.27)	1.10[a] (.45)	.53 (.48)	1.44[b] (.31)
Perceived Soviet threat measure	.02 (.39)	-.46 (.42)	-1.15 (.78)	-.14[a] (.07)	-1.60[b] (.53)
Prior-year actual controllable outlays	.07 (.09)	-.04 (.11)	.32 (.18)	.31[a] (.16)	.34[b] (.13)
Presidential revenues	.18[b] (.06)	.17[a] (.07)	.05 (.13)	.09 (.11)	.39[b] (.09)
Presidential unemployment measure	.07 (.05)	-.04 (.06)	.07 (.09)	.09 (.08)	.10 (.06)
Presidential inflation measure	-.07[a] (.03)	.02 (.03)	-.02 (.06)	-.04 (.05)	-.07[a] (.04)
Fourth year after presidential election	-.07 (.09)	.11 (.11)	-.46[a] (.19)	-.48[b] (.13)	-.42[b] (.12)
Johnson dummy variable				.59[b] (.26)	
R^2	.99	.99	.51	.63	.95
$\bar{R}^2$	.98	.99	.25	.40	.92
Durbin-Watson	1.70	2.38	1.97	2.45	2.43

Note: Standard errors in parentheses.

[a]Significant at a 95% confidence level, one-tailed test.

[b]Significant at a 99% confidence level, one-tailed test.

dependence. The revenues coefficient in column 2 is positive and significant. All three parochial defense variables have negative coefficients. The coefficient for the level of armed conflict variable is itself significant, while an F-test indicates that the three defense variables collectively exert a significant influence on uncontrollable spending at the .95 level. Within the executive branch, uncontrollable expenditures respond positively to revenues and negatively to defense and controllable spending pressures; that is, executive budgetary decision making for uncontrollable spending exhibits considerable interdependence.[18]

As in Table 1, controllable domestic programs display no interdependence within the executive branch. Indeed, the level-of-armed-conflict variable exerts a positive and significant influence on controllable domestic programs. This result, however, is probably due to the simultaneous growth of the Vietnam War and expansion of many Great Society programs. Employing post hoc analysis, the insertion of a Johnson administration dummy variable (column 4) eliminated the anomalous war coefficients. Consistent with the results in Table 1, the last column of Table 2 suggests that total spending in the president's budget is a function of both bottom-up and top-down spending pressures.

The signs of the coefficients for the economic variables throughout the specifications in Table 2 are generally consistent with those in Table 1. The absolute values of the coefficients, however, are substantially smaller. The impact on spending of inflation, relative to unemployment, also appears stronger within the executive branch.[19] The weak direct influence of projected economic conditions on executive branch spending is surprising and may reflect the long lead times between the compilation of such forecasts in the executive branch and the disbursement of funds (as much as 24 months in some cases). These results do not mean that fiscal policy pressures exert no influence on the president's budget. Both uncontrollable and defense outlays are sensitive to projected revenues. Moreover, unemployment and inflation affect projected revenues and therefore indirectly influence presidentially proposed uncontrollable and defense budgets. Nonetheless, the results suggest that discretionary fine-tuning of fiscal policy within the executive branch did not greatly affect spending in the president's budget during this period. The impact of presidential elections on presidential budgets is similar to that in Table 1, as the dummy variable for the fourth year after a presidential election acquires a negative coefficient in all of the equations but that for uncontrollable spending. Presidents seemingly wish to appear fiscally prudent in the budget that is debated during the presidential campaign, more so in defense and controllable domestic spending than in uncontrollable expenditures.

Table 3 presents the results for congressional actions on presidential budget requests, employing the same specifications as those reported in Table 2. The results for defense and controllable domestic spending are broadly consistent with Tables 1 and 2. Defense spending exhibits interdependence, while controllable domestic spending appears to be independent. In contrast to the executive branch results, however, congressional budgeting displays no interdependence in uncontrollable spending decisions. One cannot reject the hypothesis that total spending within Congress is independent of revenues. Congressional budgeting thus displays less interdependence than the executive branch, and such interdependence is confined solely to defense spending.[20]

Similarly to the results in Tables 1 and 2, congressional spending decisions in all three categories respond countercyclically to unemployment. Only congressional defense spending, however, exhibits a significant countercyclical response to inflation. Total spending responds in a countercyclical fashion to both unemployment and inflation, but the disparity between the impact of unemployment and inflation on spending is larger within Congress than in the executive branch. The variable for the fourth year after a presidential election again acquires a negative and significant coefficient, indicating that Congress is more stringent in its treatment of the president's budget during presidential election years, particularly in defense spending.

Presidential Political Variables

Presidential political ideology represents a potentially important influence on budgetary behavior and has received considerable attention in recent work.[21] Table 4 reports the results of adding both administration-specific dummy variables and, separately, a dummy variable for presidential party affiliation to a version

Table 3. Congressional Outcomes Regressed on Presidential Budget Figures

	Dependent Variable			
Independent Variables	Defense Outlays	Uncontrollable Outlays	Controllable Outlays	Total Outlays
Constant term	-.01 (.02)	-.01 (.01)	.06[a] (.03)	.04 (.03)
Presidential uncontrollable outlays	-.14 (.09)	.88[b] (.08)	.18 (.16)	.89[b] (.16)
Presidential defense outlays	.57[b] (.13)	-.15 (.11)	-.10 (.22)	.32 (.23)
Level of armed conflict	.54 (.35)	.07 (.31)	-.69 (.64)	1.30[a] (.65)
Perceived Soviet threat measure	.91[a] (.35)	-.25 (.30)	.17 (.64)	.83 (.64)
Presidential controllable outlays	-.04 (.02)	.30[a] (.13)	.21 (.29)	.47 (.28)
Congressional revenues	.29[b] (.10)	.09 (.09)	-.10 (.18)	.27 (.18)
Congressional unemployment measure	.23[a] (.10)	.13 (.09)	.18 (.17)	.54[b] (.17)
Congressional inflation measure	-.12[a] (.06)	.02 (.05)	-.11 (.11)	-.22[a] (.11)
Fourth year after presidential election	-.28[a] (.16)	-.06 (.13)	-.19 (.28)	-.53[a] (.28)
R^2	.97	.99	.30	.84
$\bar{R}^2$	.96	.99	-.07	.76
Durbin-Watson	1.96	2.70	2.07	1.89

Note: Standard errors in parentheses.

[a] Significant at a 95% confidence level, one-tailed test.

[b] Significant at a 99% confidence level, one-tailed test.

Table 4. Coefficients for Administration and Presidential Party Dummy Variables When Added to Modified Benchmark Specifications

	Uncontrollable	Defense	Controllable
Kennedy	.12 (.27)	-.02 (.32)	.09 (.34)
Johnson	-.07 (.18)	.08 (.23)	.05 (.30)
Nixon	.22 (.30)	-1.10 (.68)	-.41 (.27)
Ford	-.22 (.53)	-1.10 (.97)	.49 (.40)
Carter	-.39 (.52)	-1.50 (1.00)	.06 (.32)
Democratic President	-.16 (.11)	.08 (.16)	.15 (.21)

Note: Standard errors in parentheses. Reported coefficients and standard errors multiplied by 100.

of the specification in Table 2.[22] The coefficients for the remaining variables in each regression were not substantially altered from their respective values in Table 2 and, in the interests of space, are not reported.

The administration-specific variables and the presidential party variable are small in magnitude and statistically insignificant in all of the equations in Table 4, although the results contain weak evidence of reductions in defense spending during the Nixon, Ford, and Carter administrations. Similar results are obtained when administration-specific variables are introduced into the specifications in Tables 2 and 3. The only exception to this pattern concerns the effect (discussed above) of the Johnson administration on controllable spending in the president's budget.[23] Interacting inflation and unemployment with presidential political affiliation in these specifications also failed to reveal any significant party-based differences in the responsiveness of presidential budgets to economic conditions.

Summary and Conclusions

Previous quantitative studies of budgeting and political economy within the U.S. have been hampered by unrealistic assumptions concerning institutional structure and policy maker omnipotence, as well as a narrow focus on a single dimension of budgetary or programmatic policy to the exclusion of other factors. In this paper, we provide a framework for the integrated analysis and comparison of congressional and executive budgetary priorities, the role of top-down and bottom-up influences on spending, and the impact of political and economic variables on budgetary priorities.

The results of the tests for budgetary interdependence within Congress and the executive branch contain important implications for incrementalist theories of budgeting. The incrementalist hypothesis that individual agency allowances are independent, heretofore tested largely on the basis of the goodness of fit of nonnested models, receives little support in these results. Instead, the outcomes of the combined congressional and executive budgetary processes reflect both top-down and bottom-up spending pressures.

Budgetary interdependence appears greater within the executive branch than in Congress, judging from the results for total spending and uncontrollable spending. Budgetary outcomes within the executive branch are interdependent for both defense expenditures and uncontrollable expenditures, categories that accounted for over 80% of total noninterest outlays by 1981. Such a finding is consistent with prior studies of executive branch budgeting and contrasts with the assertions of Heclo (1985) and Schick (1985) that top-down budgeting within the executive branch is a novel phenomenon associated with the Reagan administration. Executive branch budgets have been heavily influenced by top-down pressures and policy targets during much of the postwar period.

Consistent with the critiques mentioned above, congressional budgeting displays substantially less interdependence than do executive branch budgets. Even within the congressional budget process, however, there are indications of interdependence within defense spending, as defense budgets respond to available revenues and pressures for expenditures in other programs. Why does congressional defense budgeting differ from congressional budgeting for other agencies and programs? Defense was the largest controllable budget category and represented the largest single appropriations bill during this period. An appropriations (and, implicitly, outlays) decision of such prominence may have been subject to a greater degree of reconciliation with fiscal and domestic budgetary policies than other, smaller appropriations bills. Alternatively, the size and saliency of this spending bill may have given presidents more influence on congressional decision making in defense budgeting than in other areas. Regardless of the precise explanation, this result underlies the importance of examining different spending categories separately in the analysis of budgetary decision making.

The incrementalist analysis of budgetary decision making emerges from this analysis somewhat bruised for Congress and requires considerable qualification and revision for the executive branch. The incrementalist paradigm has been tested more extensively within Congress, generally for domestic controllable programs. It is exactly here, within Congress and in domestic controllable spending in both Congress and the executive branch, that we find budgetary independence to be most significant and the incrementalist description most accurate. Our analysis, therefore, does not completely refute the incrementalist conclusion concerning budgetary independence but suggests that such independence is far from pervasive within the federal budget. Moreover, the share of total federal spending within which budgetary independence is significant in both the executive branch and Congress has declined considerably during the post-war period.

has declined considerably during the post-war period.

This study also supports a partial revision of the role and significance of uncontrollable spending. Their name notwithstanding, uncontrollable programs display substantial interdependence within executive branch budgeting, responding to the revenue environment and pressures for expenditures in other programs. Executive branch policy makers exercise some control over proposed spending for uncontrollable programs, either through legislative changes proposed with the release of the presidential budget or through the manipulation by these policymakers of economic and demographic estimates. Indeed, uncontrollable programs appear more interdependent than controllable domestic programs, consistent with the arguments of Schick (1977) and others that putatively controllable spending may be more difficult to reduce than uncontrollable spending.[24] While provocative, these results leave open the possibility that the actual level of uncontrollable expenditures may in fact be difficult to control; the *projected* level of such expenditures in the president's budget may be malleable, however, through the strategic employment of assumptions.[25]

Our empirical results also indicate that economic factors exert a major influence on budget outcomes, although the nature of the influence varies considerably across spending categories. Inflation produces countercyclical responses in defense and controllable programs but not in uncontrollable spending. Total spending responds in a weakly countercyclical fashion to inflation. Unemployment provokes a countercyclical spending response in all three spending categories. Total spending appears to respond to fluctuations in inflation and unemployment in an asymmetric fashion.

In contrast to the strong impacts of economic factors on budgetary priorities, the influence of political variables, such as the party composition of the House, party affiliation of the White House, and even administration-specific effects is modest at best. At this level of aggregation, budgetary allocations appear to be determined primarily by structural factors and interdependencies that exhibit considerable stability during this period.[26] These stable, relatively nonideological spending priorities contrast sharply with the portrayals in the political economy literature. In addition, the sensitivity of budgetary behavior to the proximity of presidential elections cited by the political business cycle literature does not exist in these data.

This analysis thus provides little support for a strictly ideological interpretation of the wellsprings of presidential budgetary and economic policy priorities within the postwar U.S. economy. The differences between Democratic and Republican budgetary policies or between administrations generally appear modest by comparison with the differences between executive and congressional policies during the postwar period.[27] Indeed, these results suggest that separating the analysis of executive budgetary behavior from that of Congress is extremely important.

As in any exercise of this sort, our findings are subject to important qualifications. This modelling framework cannot control thoroughly for strategic behavior in presidential and congressional budgeting. Budgetary priorities are measured at a high level of aggregation. This analysis cannot address the competing influence of top-down and bottom-up influences *within* such categories as domestic controllable spending. Similarly, many of the conclusions concerning the role of political influences may be sensitive to aggregation. The empirical analyses are constrained by the small number of observations and the potentially unlimited set of independent explanatory variables. Nonetheless, our empirical results appear robust to different formulations of the predetermined variables. The specifications assume a high degree of stability in budgetary priorities over the 1955–81 period. This assumption is plausible, given the stable structure of the budgetary process during this period and the significant and reasonable coefficient estimates and the high goodness of fit statistics, but it is difficult to test due to the small number of degrees of freedom.

Despite these qualifications, the empirical analysis we have presented allows the various influences on budgetary priorities in the executive branch and Congress to interact in a fashion not previously considered. Moreover, the solution to the problems we have noted is not continued reliance on the assumptions that executive and congressional budgetary priorities are the same, that observed macroeconomic outcomes are intended outcomes, or that all spending categories respond identically to fiscal policy and to political, economic, and other influences on budgetary outcomes. Instead, these difficulties underscore the importance of further comparative and disaggregated empirical research and suggest directions for future inquiry.

Appendix

The following provides a listing and definitions of the variables employed in the empirical analysis:

Variable Definitions

Congressional uncontrollable. Uncontrollable domestic outlays for fiscal year *t* following congressional action on fiscal year *t*'s budget, divided by GNP for calendar year *t*.

Presidential uncontrollable. Uncontrollable domestic outlays for fiscal year *t* as proposed in the president's budget, divided by GNP for calendar year *t*.

Actual uncontrollable. Actual uncontrollable domestic outlays for fiscal year *t*, divided by GNP for calendar year *t*.

Congressional Defense. Department of Defense (DoD) outlays for fiscal year *t* following congressional action on fiscal year *t*'s budget, divided by GNP for calendar year *t*.

Presidential Defense. DoD outlays for fiscal year *t* as proposed in the president's budget, divided by GNP for calendar year *t*.

Actual defense. Actual DoD outlays for fiscal year *t*, divided by GNP for calendar year *t*.

Congressional controllable. Controllable domestic outlays for fiscal year *t* following congressional action on fiscal year *t*'s budget, divided by GNP for calendar year *t*.

Presidential controllable. Controllable domestic outlays for fiscal year *t* as proposed in the president's budget, divided by GNP for calendar year *t*.

Actual controllable. Actual controllable domestic outlays for fiscal year *t*, divided by GNP for calendar year *t*.

Congressional revenues. Revenues (as defined in text) for fiscal year *t* following congressional action on fiscal year *t*'s budget, divided by GNP for calendar year *t*.

Presidential revenues. Revenues (as defined in text) for fiscal year *t* as proposed in the president's budget, divided by GNP for calendar year *t*.

Actual revenues. Actual revenues (as defined in text) for fiscal year *t*, divided by GNP for calendar year *t*.

Congressional inflation measure. The annual inflation rate in the government price deflator for the first six months of calendar year *t*-1.

Presidential inflation measure. The annual inflation rate projected by the executive branch for calendar year *t*-1.

Congressional unemployment measure. Change in the civilian unemployment rate from the third quarter of calendar year *t*-2 to the third quarter of calendar year *t*-1.

Presidential unemployment measure. Change in the civilian unemployment rate from the fourth quarter of calendar year *t*-2 to the level projected by the executive branch for calendar year *t*-1.

Level of armed conflict. Change in the number of U.S. servicemen killed in action between calendar year *t*-2 and calendar year *t*-1. Reported coefficients and standard errors are multiplied by 1 million.

Perceived Soviet threat. Average conflict score of messages sent from the Soviet Union to the U.S. for calendar year *t*-2 as coded in the Conflict and Peace Data Bank (COPDAB) data set. Reported coefficients and standard errors are multiplied by 1,000.

Northern House Democrats. Number of nonsouthern Democrats in the House of Representatives during calendar year *t*-1. Alabama, Arkansas, Florida, Georgia, Kentucky, Mississippi, North Carolina, South Carolina, Tennessee, Texas, Virginia, and Louisiana are coded as southern states. Reported coefficients and standard errors are multiplied by 10,000.

n^{th} year after presidential election. A dummy variable assuming a value of 1 during the n^{th} year of an administration. Reported coefficients and standard errors are multiplied by 100.

Budget figures were coded directly from the *Budget of the United States Government*. Trust fund receipts (e.g., FICA) and trust fund outlays (e.g., Social Security) were included for all years. (Prior to fiscal year 1968, trust fund activities were not included in the annual presidential budget document.) The congressional figures for fiscal year *t* were obtained from the president's budget for fiscal year *t*+1, released in January of calendar year *t*. Revenues include total projected revenues (including payroll taxes) plus receipts for oil leases on the outer continental shelf, interest paid to trust funds, and employee contributions to federal retirement programs (currently treated in the presidential budget as "offsetting receipts") minus gross interest payments. (Exclusion of interest payments from revenues is necessary for symmetry with our exclusion of interest payments on the expenditure side.) Uncontrollable programs include Medicare, Medicaid, Old Age Survivors and Disability Insurance (OASCI), Civil Service retirement benefits, railroad retirement benefits, veterans' pensions, unemployment compensation, Aid for Families with Dependent Children (AFDC), food stamps, and school lunch programs.

The congressional measure of inflation for fiscal year *t* is the rate of inflation (based on the GNP deflator) during the second and third quarters of calendar year *t*-1, a period during which Congress generally considers fiscal year *t*'s budget. The Congressional measure of unemployment for fiscal year *t* is the change in the civilian unemployment rate between December of calendar year *t*-1 and the previous December. The presidential measure of inflation for fiscal year *t* is the executive branch's projected inflation for calendar year *t*-1. The presidential measure of unemployment for fiscal year *t* is the difference between the executive branch projection for unemployment during calendar year *t*-1 and the unemployment rate during January of calendar year *t*-1. Economic forecasts for the executive branch were taken from the president's budget after fiscal 1976 and, for previous years, from internal executive branch memoranda obtained from the Office of Management and Budget and several presidential libraries. In most cases, these projections were made during November or December of calendar year *t*-2, in conjunction with the final phase of the executive budgetary process for fiscal year *t*. These variables and their collection are described in more detail in Crecine, Kamlet, Mowery, and Winer (1981). Approximately 15% of our series were missing observations. Actual magnitudes were substituted for these missing data in the reported results. Elimination of these observations entirely did not meaningfully alter our coefficient estimates. In the absence of comparable forecasts for congressional budgeting, we used for fiscal year *t* the inflation and unemployment rates for the second and third quarters of calendar year *t*-1.

The level of armed conflict measure is defined as $W_t = KIA_{t-1} - KIA_{t-2}$, where KIA_t denotes the number of U.S. servicemen killed in action in calendar year *t*. For a detailed discussion of the rationale for the use of this variable, see Fischer and Crecine (1981) and Fischer and Kamlet (1984). The perceived Soviet threat variable is drawn from the Conflict and Peace Data Bank (COPDAB) data set. This data set, compiled under the direction of Professor Edward Azar, codes occurrences between nations that are covered in any of approximately 70 major public sources, primarily newspapers. Interactions are coded in terms of the countries involved (distinguished between a sending nation and a receiving nation) and the type of issue involved (e.g., economic, military, cultural, etc.) using a fifteen-point conflict scale. The measure of perceived Soviet threat is based on the average conflict-scale ranking of all of the communications from the USSR to the U.S. coded under the military category for each calendar year. For a review of the COPDAB data set see Azar and Havener (1976), Howell (1983), Vincent (1983), and McClelland (1983).

An alternative budgetary outcome measure to that used here is the "full employment budget" (or the associated deficit or surplus), which measures total spending at some notional level of full employment in the national economy. This measure is employed in Pack (1985) and Lowery (1985). Smyth (1970) and others, however, raise a number of theoretical objections to the appropriateness of the full employment surplus as an indicator of short-term fiscal policy. In addition, there exist no disaggregated formulations of the full employment budget across budgetary spending categories, and existing estimates of full employment spending for longitudinal analysis are quite crude.

Our spending revenue variables are divided by GNP. Among recent works, Price (1978), Cameron (1978), Knott (1981), Alt and Chrystal (1983), Lowery and Berry (1983) and Hicks (1984) measure expenditures as a share of GNP. Measuring expenditure variables relative to GNP reduces heteroskedasticity problems. Other techniques, of course, can also be used to correct heteroskedasticity, for instance, a three-step estimation procedure proposed by Glejser (1969). The Glejser procedure was performed on the basic specifications reported in Table 1. Reestimating the basic specifications using this alternative correction procedure did not significantly alter the magnitude of any of the coefficients.

Estimation of the reduced-form equations is based on the assumption that the error terms in equation system (2) are normally distributed and independent of one another across time (this latter assumption is tested, see below). Identity (2e) makes one of the equations (2a) through (2d) superfluous in the sense that its parameters are completely identified, given the estimates of the other equations. Ordinary least squares on each equation individually provides an efficient estimation procedure for the equation system, given the absence of a specific structural system (Judge, Griffiths, Hill, Lutkpohl, and Lee 1985, 619). The modeling approach in this paper is substantially more general than the CALM framework of Fischer and Kamlet (1984), although this increased generality of the model proposed here comes at a cost: no longer can one infer the structural parameters of equation system (1) from the reduced-form system (2).

In testing for the independence of the error term for each equation over time, the Durbin-Watson statistic is calculated and reported for each regression. It is never in the range indicating the presence of first-order serial correlation. Strictly speaking, the lagged expenditure variables in our specifications are not lagged endogenous variables, because they measure past levels of actual spending rather than past values of the dependent variable, which is the budget allowance as proposed by the president or as acted on by Congress. Nonetheless, these variables share important statistical features with "true" lagged endogenous variables. In the presence of lagged endogenous variables, the Durbin-Watson statistic is generally biased toward 2 and is of diminished power. In the presence of lagged endogenous variables, the Durbin-h statistic, when calculable, provides the basis for an asymptotically valid test for first-order serial correlation. The Durbin-h statistic is calculable for all our regressions and in none of the regressions is the statistic greater than .64, well below the two-tailed 95% significance level of 1.96.

Our estimation of the reduced-form equations assumes that revenues are determined independently of concurrent budgetary and fiscal policy pressures. Support for this assumption is provided in Kamlet and Mowery (1986). The specifications, however, can also be estimated on the assumption that the revenue variable is endogenous, employing prior-year revenues as an instrument in two-stage least squares. This alternative procedure was employed for each of the specifications reported below, without substantially affecting our conclusions in any instance.

Notes

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1. For a recent survey of this work, see Alt and Chrystal (1983).

2. This statement by Chappell and Keech is typical of the literature: "We will assume that the president, through fiscal and monetary policies, controls real output" (1985, 13).

3. Examples of such work include models of the "political business cycle," for example, Nordhaus (1975), MacRae (1977), and Tufte (1978); models of government growth, for example, Niskanen (1971, 1975), and policy "reaction function" models, including Hibbs (1977), Beck (1982), Frey and Schneider (1978a and b, 1979), Russett (1982), Domke et al. (1983), Hicks (1984), and Lowery (1985). Exceptions are Kiewiet and McCubbins (1985a, 1985b).

4. These works focus on the atomistic structure of congressional budgetary decision making, differences between legislative and bureaucratic decision processes, differences in budgetary incentives between bureaucrats and congressional politicians, and differences in budgetary incentives between congressional and executive politicians. In a complementary vein, Ferejohn (1974), Weingast, Shepsle, and Johnsen (1982), and others argue that the decentralized and fragmented congressional decision process promotes excessive pork barrel spending.

5. The use in journalistic and scholarly analysis of the term *uncontrollable* to characterize many domestic entitlement programs also implies a lack of responsiveness of the budgets of such programs to fiscal, defense, and other domestic priorities.

6. This procedure contrasts with the bulk of the literature (including virtually all studies of congressional budgeting) that focuses on budget authority. See Mowery et al. (1980) or Kamlet and Mowery (1983, 1985a) for a more detailed discussion of budget authority and outlays in congressional and executive budgeting.

7. Estimation using ordinary least squares of equations with lagged endogenous variables yields coefficient and standard error estimates that are consistent (rather than unbiased) and the estimation procedure is only efficient asymptotically. Moreover, problems of multicollinearity often are exacerbated when lagged endogenous variables are employed as explanatory variables and lagged endogenous variables complicate testing and correcting for serial correlation (see the discussion in the appendix).

8. Lagged endogenous variables allow for the specification equations to be interpreted in the context of a partial adjustment model (see, e.g., Pindyck and Rubinfeld 1982). Nonetheless, the issue of the speed of adjustment of policies to changes in exogenous influences underlines the importance of avoiding "structural" interpretations of the coefficients of the reduced-form model. If responses to political and economic variables are consistent with a partial adjustment framework, the reduced-form coefficients will represent, among other things, the rate of adjustment of budgetary expenditure to their "desired" levels, rather than capturing the full long-term impact of exogenous variables on spending priorities.

9. Although this is a very simple model of expectation formation, Mullineaux (1980) suggests that expectations for inflation based on such extrapolation may be justified from a "rational expectations" perspective.

10. As noted above, the specific policy instru-

ments employed in such macroeconomic manipulations are typically ignored. The conclusions of these works have been disputed in other studies, e.g., Golden and Poterba (1980), Alt and Chrystal (1983), Thompson and Zuk (1983), and Lowery (1985). Apart from assuming a president capable and willing to manipulate the economy with a high degree of precision and timing, political business cycle theories typically assume, implicitly or explicitly, some form of retrospective theory of voting. The purely retrospective theory of voting has also been disputed by several recent works, for example, Chappell (1983), Chappell and Keach (1985), Lewis-Beck (1986).

11. These interdependence effects are not artifacts of dividing the spending and revenue variables by GNP, undertaken to correct for heteroskedasticity. Running ordinary least squares (OLS) on the raw spending variables (with suitably transformed nonspending, nonrevenue variables) produces comparable results; "significant" estimates of the same sign are obtained for the coefficients whose values determine the presence or absence of budgetary independence. Significance in such a context must, of course, be qualified by the fact that, given the presence of heteroskedasticity, the standard errors reported in running OLS are generally inconsistent—one of the reasons for dividing spending and revenue variables by GNP in the first place. Similar results are obtained when the concurrent GNP value is replaced by GNP lagged by one year.

12. See, for example, McCubbins (1983), Cusack and Ward (1981), and Moll and Luebbert (1980). Domke et al. (1983) report positive responsiveness of defense spending to a measure of East-West tension constructed by Goldmann and Lagerkranz (1977). This variable was not significant in our analyses; nor was the measure of Soviet military expenditures employed by Fischer and Kamlet (1984).

13. Again, the division of our dependent and explanatory variables by GNP raises the possibility that the apparent responsiveness of the dependent variable (outlays as a share of GNP) to inflation and unemployment reflects fluctuations in GNP, rather than in nominal budget outlays. As Berry (1985) has noted, either fluctuations in nominal outlays, nominal GNP, or some combination of the two may be responsible for these findings. In order to examine this issue further, we ran three specifications employing alternative formulations of explanatory and dependent variables. The first specification divided both the dependent and lagged endogenous variables by GNP_{t-1}; the second, in addition, divided inflation and unemployment by GNP_{t-1}; and the third used raw outlay figures (undivided by any GNP figure), multiplying the economic variables by GNP. In all cases, the nature of the impact of unemployment and inflation on budgetary outcomes is comparable to that reported in Table 1. See Kamlet and Mowery (1985b) for a more detailed discussion of this issue.

14. See Kamlet and Mowery (1985b) for the derivation of this and other figures relating to relative impacts of inflation and unemployment fluctuations on total spending.

15. Kiewiet and McCubbins (1985b) find that congressional elections exercise a positive influence on budget authority allowances for controllable, particularistic programs, which are not examined separately in our aggregate analysis. If this contressional election-based spending cycle were true, we would anticipate the coefficients for the variables representing two and four years after a presidential election to be negative and of comparable magnitude and that for three years after a presidential election to be zero in the regressions of congressional outcomes on prior-year variables (Table 1) and in the regressions of congressional outcomes on presidential budget figures. In neither case is the coefficient for the third year after a presidential election significantly less than that for the second year after a presidential election for any of the individual spending categories or for total spending. It may be that the effect of congressional election years on controllable, particularistic programs washes out when they are combined with other programs. Or, there may be different election year behavior for outlays (employed here) and budget authority (which Kiewiet and McCubbins use), implying a political business cycle that operates through greater generosity in budget authority, possibly in response to more effective interest group pressure during an election year, while outlays are restrained. The possibility of such budget authority-outlays side payments requires further research, but it suggests a very different model of the political business cycle in which interest group pressures, rather than the desire for economic stimulus, drive spending fluctuations.

16. Kiewiet and McCubbins (1985b) obtain a significant effect for House Democrats, although only for non-public works agencies in an analysis that employs budget authority rather than outlays.

17. The specification differs from that in Table 1 by the omission of northern House Democrats and two of the election year proximity variables as independent variables, due to their lack of statistical significance in Table 2. A simple F-test, analyzing the joint effect of the exclusion of these variables on the explained variance, suggests that their exclusion does not significantly affect the goodness of fit of the equations. This exclusion also did not affect the magnitude of any of the estimated coefficients in the equations.

18. For several reasons, the influence of uncontrollable spending pressures and the revenue environment on uncontrollable spending are not simply artifacts of the positive growth trends of uncontrollable spending and revenues and the

negative time trend in defense spending (measured as shares of GNP). The level of armed conflict (WAR) variable exhibits no time trend yet acquires a negative and significant coefficient in the uncontrollable regression. Similarly, the revenue variable has a positive coefficient in the defense spending regression, despite the opposite direction of the growth trends for revenues and defense spending. It is true that substantial collinearity is present between some of the independent variables, a problem that plagues many budgetary models. However, such collinearity does not produce biased coefficient estimates, only larger standard errors. That variables in this analysis have statistically significant coefficients implies that their separate impacts on the dependent variable are determined with a high degree of confidence, despite the presence of collinearity.

19. Similar results were obtained when contemporaneous inflation and unemployment measures rather than projections are used and when absolute, rather than first difference, measures of unemployment are used. These results are similar to those in Kiewiet and McCubbins (1985b), who also found Congress to be more responsive than the executive branch to economic fluctuations in budgetary formulation.

20. Two tests were undertaken to determine whether the Congressional Budget Reform and Impoundment Control Act of 1974, a key goal of which was the enforcement of greater top-down fiscal pressure and interdependence within congressional budgeting, had any impact on congressional budgetary outcomes. The specifications in Table 3 were reestimated for the pre-1974 period; no significant changes in the sign or magnitude of any coefficients were observed. In a second test, a dummy variable assuming a value of 1 for the post-1973 period and 0 elsewhere was interacted with the revenues and other spending terms in each specification. Once again, no significant results were obtained. This lack of impact is consistent with the conclusions of other works that have argued that the Congressional Budget Act has had a modest impact at best on congressional budgetary outcomes. Kamlet and Mowery (1985a) review this literature.

21. Hibbs (1977, 1979), for instance, argues that Democratic presidents place greater weight on reducing unemployment (and less on reducing inflation) than Republicans, while Beck (1982) contends that differences among presidents in their commitment to reducing unemployment are administration-specific, rather than a function of presidential political affiliation. Kiewiet and McCubbins (1985b) report a positive effect of Democratic presidents on controllable spending in the president's budget. Browning (1984), however, suggests that entitlement spending has risen most dramatically under Republican presidents.

22. The specification from Table 2 was modified by dropping insignificant variables. The *F*-test for the joint significance of the excluded variables suggested that this exclusion did not affect the explained variance. In addition, the estimated coefficients for the variables that were retained were not affected substantially by dropping these variables. Exclusion of these nonsignificant variables is likely, if anything, to bias our results in favor of accepting the hypothesis that administration effects are significant.

23. The overall pattern of insignificance also remains when administration dummy variables are constructed as spikes, with a 1 in the first year of the administration and a 0 elsewhere, to allow for a one-time impact of a new administration on spending priorities. Placement of the spike in the second or third year of the administration also yields insignificant results.

24. "The label 'uncontrollable' is simply a shorthand way of identifying items which cannot be controlled without changes in the law. But the label veils a significant paradox of budgeting: most of the 'uncontrollables' can be controlled, but most of the 'controllables' really cannot" (Schick 1977).

25. The period examined is one during which uncontrollable expenditures increased relative to both the rest of the budget and GNP. As such, these results do not speak to the possibility of *reductions* in uncontrollable expenditures in response to other budgetary and fiscal spending pressures, although in fact every president since Nixon has proposed reductions in some uncontrollable programs (see LeLoup 1978).

26. An interesting question for future investigation concerns the extent to which the Reagan administration represents a departure from this consensus on budgetary priorities during the 1955–1981 period.

27. Similar conclusions have been reached by students of another central element of the U.S. political economy, trade policy. Baldwin and Richardson note that

> the general consistency of U.S. trade policy over time is all the more remarkable given the frequent change of political party in power, especially in the Executive branch, but also in the Congress. Party affiliation, in fact, seems no longer to be a useful predictor of U.S. trade initiatives. A more useful predictor appears to be some measure of Executive versus Congressional control. The two branches of U.S. government have different outlooks on trade policy due to differences in constituencies. Conflict has punctuated relations between branches of government much more often than between political parties. (1984, 4)

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AGENDAS AND THE CONTROL OF POLITICAL OUTCOMES

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A considerable theoretical literature argues that if everyone votes sincerely, then an agenda setter has near dictatorial influence on final outcomes, whereas if everyone votes strategically, then an agenda setter's power is considerably reduced. This literature assumes that all feasible agendas are of a special type called amendment agendas. *But actual legislative and committee agendas—notably those found in Congress—often are not of this type. Once we expand the domain of feasible agendas to include all types allowed by common parliamentary practice, the influence of agendas on legislative outcomes expands, even with strategic voting. Besides showing with counterexamples that previous results do not extend to a more realistic domain of agendas, we prove some theorems that specify the limits (such as they are) of an agenda setter's power.*

Beginning with Farquharson (1969), a growing literature has investigated the interplay between agenda structure and voting strategy in legislatures and committees by examining an agenda setter's ability to manipulate outcomes and the ability of strategic voters to offset an agenda setter's power. The results of these investigations appear to support the following picture. If voters act *sincerely* rather than *strategically*—if at every vote they express their true preferences between alternatives instead of looking ahead to final consequences—an agenda setter is a near dictator: the set of outcomes achievable by different agenda forms is comparatively large (McKelvey 1979, Miller 1980, Schwartz 1986). But if voters act strategically, an agenda setter's power is not so great: the set of achievable outcomes is generally smaller (Banks 1985, Miller 1980, Shepsle and Weingast 1984).

Some have taken such results to measure the extent to which legislative decisions are at the mercy of elites who control agendas (Riker 1982). But even if no individual or small group controls agendas—even if the agenda-setting process is open and democratic—the power of a *hypothetical* agenda setter is important for political analysis. It is important because it reflects the degree to which outcomes depend on procedural structure rather than votes, hence the degree to which the real legislative game occurs at the agenda-setting stage rather than the voting stage.

The utility of these results is impaired, however, by the unrealistic assumption that feasible agendas are all of a special type called *amendment agendas.* Such agendas work as follows: a sequence of alternatives is given, and a vote is taken between the first two, after which the winner is pitted against the third alternative, then the winner in this second

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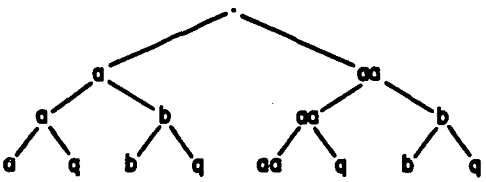
Figure 1. A Four-Alternative Amendment Agenda



ballot against the fourth alternative, and so on. Suppose, for example, that just three motions are on the floor of the U.S. House or Senate: a bill, an amendment to the bill, and an amendment to the amendment. Then there are four possible outcomes:

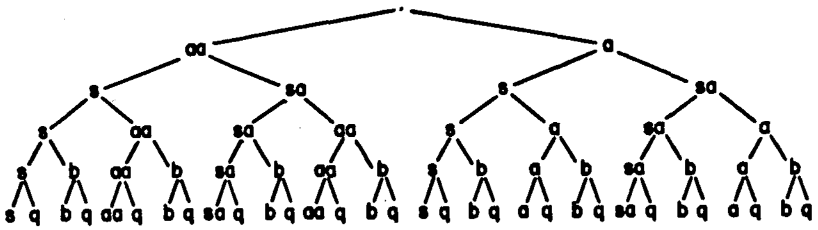
q: the status quo ante
b: the bill unchanged
a: the bill changed by the original amendment
aa: the bill changed by the amended amendment

For these alternatives, congressional procedure requires an amendment agenda in which the first vote is between *a* and *aa* (whether to amend the amendment), the second between the winner and *b* (whether to amend the bill), and the third between the winner and *q* (whether to pass the final form of the bill). We represent this agenda by the tree in Figure 1.

Not all realistic agendas are of this sort, however. Suppose, as in Riker's (1958) account of the 1953 Agricultural Appropriations Act, that the following motions are introduced in the U.S. House: a bill, an amendment to the bill, an amendment to the amendment, a substitute amendment, and an amendment to the substitute. Rule XIV of the House requires that members perfect the amendment and the substitute amendment before deciding whether to substitute and then whether to amend the bill. Hence, the agenda, shown in Figure 2, first pairs the bill perfected by the original amendment (*a*) with the bill perfected by the amended amendment (*aa*). But then, unlike an amendment agenda, this one does not pair the winner of the first ballot with anything in the second stage. Instead, the bill perfected by the substitute amendment (*s*) is paired with the bill perfected by the amended substitute amendment (*sa*). The next vote pairs the winners in these first two votes, and the winner in this vote is then paired with *b*. The final vote pits the status quo (*q*) against the survivor of all previous votes—or this vote is postponed until other sections of the bill have been considered (see Bach 1981, Sullivan 1984).

The difference between the agendas in Figures 1 and 2 is subtle. In Figure 1 the winner at one stage enters the balloting in the next, but in Figure 2 *s* and *sa* are paired in the second ballot regardless of which alternative wins in the first. Are such differences in form merely stylistic, or do they affect the power of agendas and agenda setters and the efficacy of strategic voting?

Figure 2. A Congressional Agenda with Substitutes


aa
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Our principal conclusion is that by focusing on amendment agendas, the literature conveys an inaccurate picture. Quite a variety of agendas is available in Congress and other institutions, and few of the theorems about amendment agendas apply generally. The critical assumption of previous results concerning the sensitivity of outcomes to agendas is not that voters are strategic or sincere but that all feasible agendas are amendment agendas. One might suppose that results about amendment agendas fill in part of the map, applying to an important class of procedures. The assumption commonly made, however, is not that the actual agenda is of the amendment type nor that the set of feasible agendas contains amendment agendas but that this set contains *only* amendment agendas, and that assumption is rarely true.

After surveying common agenda forms in the first section, we draw some structural distinctions among agendas in the second section, then define strategic and sincere voting in the third section. The fourth section is a review of previous results based on the amendment-agenda assumption. In the fifth section we show with counterexamples that these results no longer hold when the amendment-agenda assumption is relaxed to allow for other agenda forms. In the sixth section we state several theorems (proved in the Appendix) about the actual limits of a hypothetical agenda setter's power under sincere and strategic voting.

Examples of Alternative Agenda Forms

Figure 2 depicts a common type of congressional agenda, but even more complex agendas are possible (see Bach 1981). Suppose the following eight motions are introduced: a bill, an amendment to perfect the bill, an amendment to perfect the amendment, a substitute amendment, a substitute bill, an amendment to perfect the substitute bill, a substitute for the substitute bill, and an amendment to perfect the latter substitute. These motions give rise to nine outcomes:

q: the status quo
b: the bill unchanged
a: the bill perfected by the amendment
aa: the bill perfected by the amended amendment
sa: the bill perfected by the substitute amendment
sb: the substitute bill
asb: the amended substitute bill
ssb: the substitute for the substitute bill
assb: the amended substitute for the substitute bill

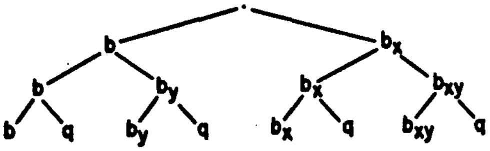
The voting order is as follows (Sullivan 1984):

1. *a* vs. *aa*
2. winner of 1 vs. *sa*
3. winner of 2 vs. *b*
4. *sb* vs. *asb*
5. *ssb* vs. *assb*
6. winner of 4 vs. winner of 5
7. winner of 3 vs. winner of 6
8. winner of 7 vs. *q*

We recoil from drawing the tree for this agenda, but note that this and the previous example both fail to qualify as amendment agendas because of *discontinuities*—steps at which the winner of the previous vote does not enter the balloting but is set aside for later consideration. In the last example, the alternatives paired in the fourth and fifth ballots are independent of all previous winners.

The existence of discontinuities is not the only way agendas in Congress and other institutions can differ from amendment agendas. Notice that the agendas in Figures 1 and 2 share two properties: First, they are *symmetric*—that is, each tree splits into two subtrees that are alike except for the interchange of alternatives (*a* and *aa*) occupying their top nodes;

Figure 3. A Two-Period Congressional Agenda



each of these subtrees splits in the same way, and so on down the agenda. Second, each is a *one-period* agenda—that is, all motions are on the floor before any are voted on. In a multiperiod agenda, by contrast, some motions are voted on before others are recognized. For example, suppose that, in addition to *b* and *q*, we also have

b_x: the bill perfected by amendment *x*
b_y: the bill perfected by amendment *y*
b_{xy}: the bill perfected by both amendments

In this case standard parliamentary procedure requires a two-period agenda in which *x* is voted up or down before *y* is recognized. Depicted in Figure 3, this agenda requires three comparisons between alternatives:

1. b vs. b_x
2. $\begin{cases} b \text{ vs. } b_y \text{ if } b \text{ wins at 1} \\ b_x \text{ vs. } b_{xy} \text{ if } b_x \text{ wins at 1} \end{cases}$
3. winner of 2 vs. *q*

Besides being multiperiod, the agenda in Figure 3 differs from those in Figures 1 and 2 in that it is not symmetric. The tree branches into two subtrees that are dissimilar even apart from the interchange of *b* and b_x: one contains b_y where the other contains b_{xy}. This is because either b_y or b_{xy} enters the balloting at the second round, but *which* of them does so depends on which alternative—*b* or b_x—wins at the first round. We call the agenda in Figure 3 a *first-order* agenda because it involves only first-order amendments (amendments to the original motion) and because these amendments are compatible (one outcome can contain all of them). Every first-order agenda is continuous, but it is not symmetric unless it involves just one amendment.

The agendas examined thus far are *uniform*, by which we mean that all branches in their trees have the same length. But even uniform agendas do not exhaust the realistic types. Consider a *sequential-elimination* agenda, in which a new motion is introduced and voting proceeds to the next stage only if the current motion fails. Illustrated by Figure 4, such agendas are neither uniform nor symmetric. In Farquharson's celebrated narrative (1969), Pliny sought to discourage a sequential-elimination agenda in favor of the plurality vote. Millennia later, such agendas are observed when parliamentary bodies such as the U.S. Senate vote on personnel questions or when members are obliged to take some action (to adopt a budget, say) and so must keep voting on proposed actions until one passes.

Other examples of nonuniform and hence nonsymmetric agendas include those with a succession of "killer amendments," as in Enelow and Koehler's discussion of the Panama Canal Treaty (1980). Opponents of the treaty introduced a series of seemingly innocuous amendments solely to make the treaty's wording differ in some way from the text that had been negotiated with Panama. If any one of these motions had passed, the

Figure 4. A Sequential-Elimination Agenda

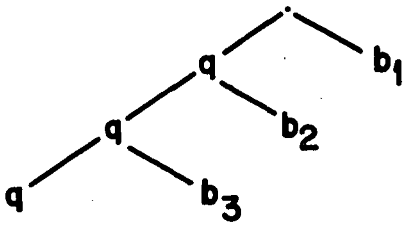
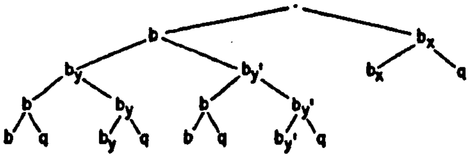


Figure 5. A Hybrid Agenda



others would not have been introduced. We can illustrate this situation with two first-order amendments and, for good measure, a second-order amendment. The agenda, depicted in Figure 5, comprises five alternative outcomes:

q: the status quo
b: the bill
b_x: the bill amended by x
b_y: the bill amended by y in its original form
$b_{y'}$: the bill amended by the amended version of y

Neither symmetric nor uniform nor continuous, this hybrid agenda suggests that our examples only begin to uncover the various forms of agendas that should interest political scientists. At any rate, they show that amendment agendas do not exhaust the possibilities.

Our ability to formulate nonamendment agendas raises the question, how prevalent are amendment agendas in practice? Although two-motion agendas can be of the sequential-elimination type, the most commonly observed two-motion agendas involve an initial motion and one amendment, and they are of the amendment variety. But consider agendas based on three motions: a bill, b, and two amendments, x and y, where x is recognized before y. If the moving of y depends on whether x passes, we cannot have an amendment agenda. So suppose y is moved regardless of the action taken on x. Then if y is a second-order amendment (Figure 6a) or a complete substitute for b (Figure 6b), the agenda is of the amendment type. But if y is a first-order perfecting amendment (Figure 6c), the agenda is not an amendment agenda. Hence, an agenda based on three motions is an amendment agenda only if (1) the third motion is made and recognized regardless of whether the second passes *and* (2) the second motion is either a second-order amendment or a complete substitute.

First-order agendas, like that of Figure 6c, are arguably the most common in most committees. The impression, however, that amendment agendas are especially prevalent might arise from a confusion between *motions* and *alternatives*. For example, suppose there is an initial motion, to pass bill b, and two first-order perfecting amendments, x and y. If the second motion is recognized regardless of the action taken on the first, then the usual language of committee deliberations, which makes reference to motions rather than final outcomes, leads to the impression that the agenda is that of Figure 6d, an amendment agenda. However, because x and y are compatible motions rather than alternative outcomes, the correct agenda is the one shown in Figure 6c, which is not an amendment agenda.

A Classification of Agendas

In this section we formally define several important categories of agendas, most of them informally introduced in the

Figure 6. Contrasting Agendas

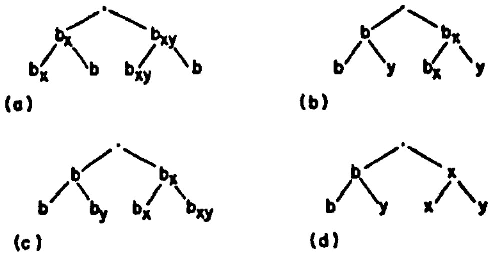
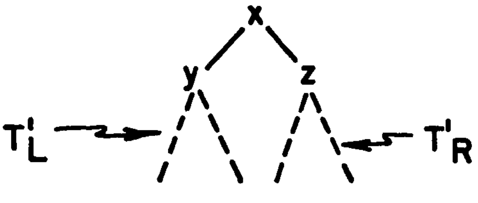


Figure 7. Agenda T′



previous section. We begin by letting X be a set of alternative outcomes, letting x, y, z, a, b, and so forth denote elements of X, and letting α, β, and so forth denote finite, nonempty subsets of X. We represent agendas by binary trees:

> DEFINITION. *An* agenda *on α is a finite binary tree T such that (1) every node (vertex) of T is occupied by some member of α, (2) every member of α occupies some bottom node of T, and (3) $y \neq z$ for every subtree T' of T of the form shown in Figure 7.*

Condition (1) requires that every fork in the tree represent a choice between two alternatives and (as a mathematical convenience, which we disregard in most figures) that even the top node be occupied by some alternative (an arbitrary one will do). Condition (2) ensures that there is some path to every outcome in α, and (3) rules out "nonchoices" of the form y *vs.* y.

If an agenda T' has more than one node, then it has at least three *subtrees:* T' itself and its *left* and *right principal subtrees,* T'_L and T'_R, shown in Figure 7. Note that a principal subtree with more than one node splits into its own principal subtrees.

Now consider the two agendas in Figure 8. They are the same except for the left-right order of subtrees and alternatives at various nodes. Because they embody the same sequence of comparisons and must yield the same outcome, we call them *equivalent*. Here is a recursive definition of this relation:

> DEFINITION. *If the Agenda T_1 has but one node, then T_1 is* equivalent *to T_2 if and only if $T_1 = T_2$. If T_1 has more than one node, then T_1 is equivalent to T_2 if and only if one principal subtree of T_1 is equivalent to one principal subtree of T_2, while the other principal subtree of T_1 is equivalent to the remaining principal subtree of T_2.*

To develop our classification of agendas, we begin with a type exemplified by the agendas in Figures 1 and 2:

> DEFINITION. *A* symmetric *agenda on α is an agenda T on α such that, for every subtree T' of T of the form shown in Figure 7. T'_L is equivalent to the result of substituting y for z in T'_R, and T'_R is equivalent to the result of substituting z for y in T'_L.*

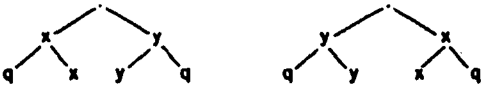
All symmetric agendas have three other important properties. First, they are nonrepetitive, a property commonly required by legislative rules. This means that they prevent an alternative once rejected from coming up again for a vote.

> DEFINITION. *A* nonrepetitive *agenda on α is an agenda T on α such that, for every subtree T' of T of the form shown in Figure 7, y does not occur in T'_R and z does not occur in T'_L.*

Second, symmetric agendas on α are *complete* in the sense that they guarantee every alternative in α its day in court: whether an alternative gets to be voted on at a given stage does not depend on the choice made at the previous stage.

> DEFINITION. *A* complete *agenda on α is an agenda on α such that, for every subtree T' of the form shown in Figure*

Figure 8. Equivalent Agendas



7, T'_L contains every alternative occurring in T'_R with the possible exception of z, and T'_R contains every alternative occurring in T'_L with the possible exception of y.

Figures 1 and 2 depict complete agendas; Figures 3–5, incomplete ones. Although all symmetric agendas are complete, not all complete agendas are symmetric. For example, the following agenda is complete but not symmetric: x and y are paired; if x wins, it is paired with z and the winner with w; but if y wins, z and w are paired and the winner paired with y.

The third property that symmetric agendas satisfy is uniformity:

DEFINITION. *A* uniform *agenda on α is one whose branches are all of the same length.*

Figures 1 and 2 depict symmetric, hence uniform, agendas; Figure 3, a uniform, nonsymmetric agenda; and Figures 4 and 5, nonuniform agendas.

The following theorem (proved in the Appendix) summarizes the properties of symmetric agendas:

THEOREM 1. *All symmetric agendas are nonrepetitive, complete, and uniform.*

A fifth type of agenda, introduced in the previous section, does not encompass all symmetric agendas:

DEFINITION. *A* continuous *agenda on α is an agenda T on α such that, for every subtree T' of T of the form shown in Figure 7, $x = y$ or $x = z$.*

Figure 1 depicts an agenda that is symmetric and continuous; Figure 2, symmetric but not continuous; Figures 3 and 4, continuous but not symmetric; and Figure 5, neither.

A final category, included in all five preceding ones, is that of amendment agendas:

DEFINITION. *An* amendment agenda *is an agenda that is equivalent to $A(x_1, \ldots, x_n)$ for some $x_1, \ldots, x_n$, where $A(x_1, \ldots, x_n)$ is defined by the following recursion: $A(x_1)$ is the 1-node tree consisting of x_1. $A(x_1, x_2, \ldots, x_{k+1})$ is the tree T in which x_1 occupies the top node and $T_L = A(x_1, x_3, \ldots, x_{k+1})$ while $T_R = A(x_2, x_3, \ldots, x_{k+1})$.*

The intuition here is that an amendment agenda is determined by a sequence $(x_1, \ldots, x_n)$ such that x_1 is first pitted against x_2. If x_1 wins, we move into the subtree T'_L, which is determined by the same sequence, less x_2; but if x_2 wins, we move into T'_R, which is determined by $(x_2, x_3, \ldots, x_n)$.

Amendment agendas are nonrepetitive, complete, uniform, and continuous. All symmetric agendas have the first three properties, but

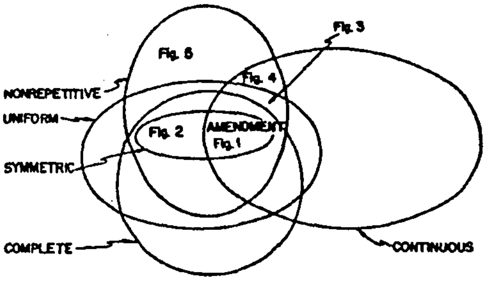
THEOREM 2. *All and only amendment agendas are symmetric and continuous.*

The Venn diagram in Figure 9 summarizes our classification, Theorems 1 and 2, and the easily proved fact that all complete and nonrepetitive agendas are uniform. (Although we have not supplied examples for every cell, it is easy to do so.)

Sincere and Strategic Voting

To define the choices from a given agenda under sincere and strategic voting,

Figure 9. Classification of Agendas



let P be a "social preference" relation on X. We assume that P is *asymmetric* (if $x\,P\,y$ then not $y\,P\,x$) and *connected in* X (if $x, y \in X$ and $x \neq y$ then $x\,P\,y$ or $y\,P\,x$). Under majority rule, $x\,P\,y$ if more people prefer x to y than prefer y to x. For the sake of generality, however, we do not require that P be the majority-preference relation. Our one restrictive assumption (common to most of the literature) is connexity, which rules out ties. Although some tie-breaking rules, such as the rule letting a chairman cast the deciding vote, can be thought of as built into P, others cannot because they depend on the positions occupied by tied alternatives in an agenda.

We can now define the *sincere* and *strategic choices*, $SIN(T)$ and $Strat(T)$, from an arbitrary agenda T. If T contains just one alternative, x, then $SIN(T) = STRAT(T) = x$. Otherwise, let x occupy the top node of T_L and y the top node of T_R. Then

$$SIN(T) = \begin{cases} SIN(T_L) \text{ if } x\,P\,y \\ SIN(T_R) \text{ if } y\,P\,x \end{cases}$$

and

$$STRAT(T) = \begin{cases} STRAT(T_L) \text{ if } STRAT(T_L) \\ \quad P\ STRAT(T_R) \\ STRAT(T_R) \text{ otherwise.} \end{cases}$$

These recursive definitions define the sincere and strategic choices from a given complex tree in terms of choices from its less complex components. The definition of $STRAT(T)$ is equivalent to the definitions of Moulin (1979) and McKelvey and Niemi (1978).

Choice Sets and Theorems about Amendment Agendas

This section reviews what is already known about the sincere and strategic choices from amendment agendas. Since P is connected, it is obvious that every α must contain either a unique dominant alternative or a unique dominant cycle, where

DEFINITION. *x is a* dominant alternative *in α if and only if $x \in \alpha$ and $x\,P\,y$ for all $y \neq x$ in α. β is a* dominant cycle *in α if and only if, for some $x_1, \ldots, x_n$, $\beta = \{x_1, \ldots, x_n\} \subseteq \alpha$, $x_1\,P\,x_2 p \cdots P\,x_n\,P\,x_1$, and $x_i\,P\,y$ for all $x_i \in \beta$ and all $y \in \alpha - \beta$.*

Now let

$$G(\alpha) = \{x \in \alpha \mid x \text{ is a dominant alternative in } \alpha \text{ or } x \text{ belongs to the dominant cycle in } \alpha\}.$$

Miller (1980) proves:

$$G(\alpha) = \{x \mid x = SIN(T) \text{ for some amendment agenda } T \text{ on } \alpha\}. \tag{1}$$

More generally:

$$G(\alpha) = \{x \mid x = SIN(T) \text{ for some symmetric agenda } T \text{ on } \alpha\}. \tag{2}$$

The latter result follows from a still more general result by Schwartz (1986, chap. 6), which allows ties and nonbinary choices. Thus, if a dominant alternative exists among the alternatives appearing on an amendment or other symmetric agenda, that alternative will be chosen. If, on the other hand, a dominant cycle exists, then some outcome in the cycle must be chosen, and any outcome can be chosen with some admissable agenda.

A third resullt follows from McKelvey's proof (1979) that for majority rule and the usual spatial context (Euclidean or, more generally, differentiable and convex preferences), if there is no undominated point, the entire space is a cycle ($x \in X$ is undominated if $y\,P\,x$ for no $y \in X$): If x and y are *any* two points in X, there is an amendment agenda T on some finite subset of X such that x is the first alternative in T (x occupies the top nodes of T and T_L) and $y = SIN(T)$. (Although the relation, M, of majority preference is not connected, we can interpret P as the result of adding some tie-breaking rule to M.)

For strategic voting, Miller's definitions (1980) of the covering relation and the uncovered set are crucial. Stated for an arbitrary subset Y of X,

> DEFINITION. *x covers y in Y if and only if $x, y \in Y$, $x \; P \; y$, and for all $z \in Y$, if $y \; P \; z$ then $x \; P \; z$. And*
>
> $U(Y) = \{x \in Y \mid \text{nothing covers } x \in Y\}$ (*the* uncovered set *of* Y).

Miller proves:

$$STRAT(T) \in U(\alpha) \text{ if } T \text{ is an amendment agenda on } T. \quad (3)$$

The significance of this result derives from these properties of U:

$$U(\alpha) \subseteq G(\alpha) \text{ and } U(\alpha) \subseteq \text{Pareto set of } \alpha.$$

Since the sincere choice from an amendment agenda on α can be anything in $G(\chi)$—often a large subset of α that includes Pareto inefficient outcomes—and since $U(\alpha)$ and hence any strategic choice from α is necessarily in the Pareto set, strategic voting has been interpreted as a means whereby voters can limit the power of an agenda setter.

Banks (1985) shows that strategic voting can limit outcomes further. His result requires three definitions:

> DEFINITION. *A vector* $\mathbf{x} = (x_1, x_2, \ldots, x_n)$ *is a* chain *in α if and only if $x_1 \in \alpha$ for all i and $x_i \; P \; x_j$ whenever $i < j$. A* maximal chain *in α is a chain $\mathbf{x}$ in α such that there is no longer chain in α containing every member of $\mathbf{x}$. And*
>
> $B(\alpha) = \{x \mid x = \textit{first element of some maximal chain in } \alpha\}$.

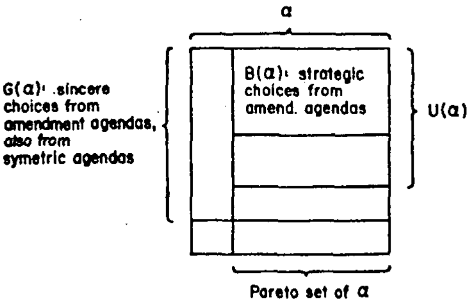
Banks proves:

$$B(\alpha) = \{x \mid x = STRAT(T(\text{ for some amendment agenda } T \text{ on } \alpha\} \subseteq U(\alpha). \quad (4)$$

We summarize results (1)–(4) in Figure 10. Notice that these results relate the final outcome only to the other alternatives on the agenda: they say nothing about alternatives in X but outside α. Hence, they do not say whether McKelvey's theorem holds for strategic voting—whether agendas can "lead anywhere" in a Euclidean space when voters act strategically. Shepsle and Weingast (1984), however, extend (3) for simple majority rule in the spatial context to show that if x_1 is the first alternative in an amendment agenda T on α and if x_j is chosen over x_k whenever $j > k$ and x_j and x_k tie, then x_1 does not cover $STRAT(T)$ in X. That is, the only points that can be reached, under strategic voting and an amendment agenda, from some initial alternative x_1 are points that x_1 does not cover. McKelvey (1986), moreover, offers a model of endogenous amendment-agenda formation in which outcomes outside the uncovered set of the entire space cannot be reached. Strategic voting, then, *appears* to limit the power of an agenda setter regardless of whether our focus is on a fixed, finite set of alternatives that must enter the agenda or an entire issue space and hence regardless of whether the agenda setter can control only the order of voting or the set of feasible alternatives as well. We say "appears" because the assumption that all permissible agendas are of the amendment type is essential to these results.

Were the set of feasible agendas restricted to amendment agendas, then strategic voting also would limit the *length* of agendas required to secure reachable outcomes. With sincere voting, if an alternative y is required to enter the voting in the first stage of an amendment agenda T on α, then there exists an amendment agenda on part or all of α by which any given outcome $x \in G(\alpha)$ can be

Figure 10. Summary of Amendment-Agenda Results


α
G(α): sincere choices from amendment agendas, also from symetric agendas
B(α): strategic choices from amend. agendas
U(α)
Pareto set of α

reached. With strategic voting, on the other hand, Miller (1980) shows that if $x \in G(\alpha)$ can be reached at all, then there exists a one- or two-stage amendment agenda on a subset of α that begins with y and yields x. Shepsle and Weingast (1984) establish a parallel result for spatial preferences. They show that all the outcomes that can be reached from x_1 (namely, the outcomes that do not cover x_1) can be reached by some one- or two-stage agenda (which may, however, contain an outcome foreign to the original agenda).

Counterexamples to Extensions

What we want to show now is that the results we reviewed in the previous section hold only for institutions in which the feasible agendas are limited to amendment agendas. That is, the apparent limits that strategic voting sets on a hypothetical agenda setter's power depend critically on the set of permitted agendas, and if we properly extend a setter's strategies to include all agendas that we might actually observe in a legislature, strategic voting is not the constraint it was thought to be.

Example 1. To see that things can be different when we turn from amendment agendas to other agenda forms, we note first that *agendas can be found from which the sincere choice, SIN(T), need not belong even to G(α).* Suppose *every* voter ranks the five alternatives shown in Figure 3 as follows: b_y, b_{xy}, b_x, b, q. Since this order is unanimous, we may suppose that P orders the alternatives the same way. Hence, if everyone votes sincerely in the agenda—call it T—of Figure 3, then b_x defeats b, after which b_{xy} defeats b_x and then q, so $SIN(T) = b_{xy}$. But b_y is the dominant alternative, so $SIN(T) \notin G(\alpha) = \{b_y\}$.

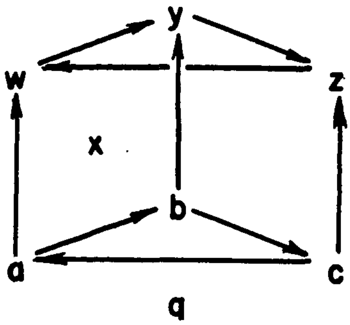
Example 2. Although Example 1 shows that outcomes outside $G(\alpha)$ can prevail under sincere voting, notice that strategic voting in this example yields b_y, which, because it is dominant, is perforce the sole element of $U(\alpha)$ and $B(\alpha)$. But suppose the following eight alternatives are on the floor of the U.S. House:

w: an unamended bill
x: the bill perfected by an amendment
y: the bill perfected by an amended amendment
z: the bill perfected by a substitute amendment
a: a substitute bill unchanged
b: a substitute for the substitute bill
c: the substitute substitute perfected by an amendment
q: the status quo

Then the agenda is as follows (Sullivan 1984):

1. x vs. y (whether to amend the amendment)
2. the winner at 1 vs. z (whether to substitute for the [perfected] amendment)
3. the winner at 2 vs. w (whether to amend the bill)
4. b vs. c (whether to amend the substitute substitute bill)
5. the winner at 4 vs. a (whether to substitute for the [perfected] substitute bill)
6. the winner at 5 vs. the winner at 3

Figure 11. Social Preference for Example 2



(whether to replace the [perfected] bill by the surviving substitute bill)

7. the winner at 6 vs. q (whether to pass the bill's surviving form)

Suppose the social preference under majority rule takes the form shown in Figure 11, where, for those alternatives not paired by an arrow, their height on the page represents P. Because the agenda tree is especially complex, we do not reproduce it here, but note that $STRAT(T) = x$ even though $x \notin B(\alpha)$. That is, chains with x as the first element include (x), (x,b), (x,b,c), (x,b,c,q), and so forth, but none of these chains is maximal since we can set w, z, or y at the top of any of them and still have a chain. Thus, *whereas an amendment agenda over the alternatives in this example must yield a point in B(α) with strategic voting, the agenda prescribed by Rule XIV yields an outcome outside B(α).* This shows that Banks's theorem about amendment agendas does not extend to other agenda forms, even realistic symmetric ones.

Example 3. Although in Example 2, $x = STRAT(T)$ is not in $B(\alpha)$, x *is* in the uncovered set, $U(\alpha)$: there is no alternative that defeats x while also defeating everything x defeats. Nevertheless, we can construct a counterexample to the proposition that the strategic choice from a realistic agenda—again a symmetric agenda permitted by Rule XIV—must belong to the uncovered set. There are seven alternatives:

c: an unamended bill
a: the bill perfected by an amendment
b: the bill perfected by a substitute amendment
x: a substitute bill unamended
y: the substitute bill perfected by an amendment
z: the substitute bill perfected by a substitute amendment
q: the status quo

Agendas with both a perfecting amendment to a bill and a substitute bill may be rare in the House. Bach (1981), however, illustrates a similar agenda with the case of Senate Bill 7, the Veterans' Health Care Amendments of 1979. The required congressional agenda is the following:

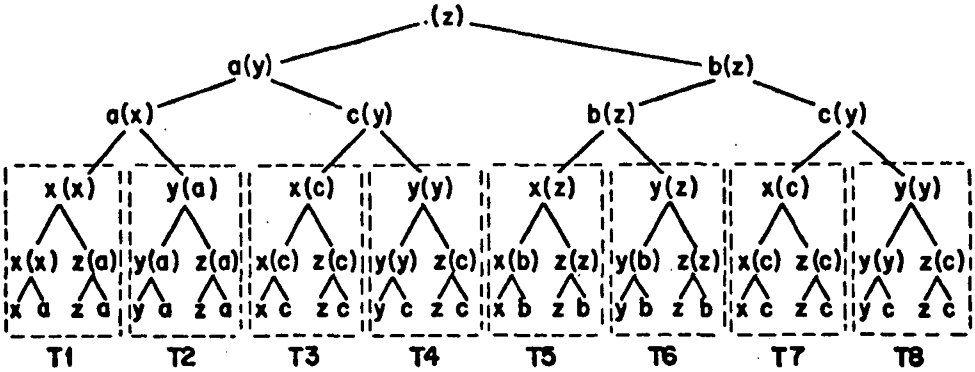
1. a vs. b (whether to replace the amendment with a substitute amendment)
2. winner at 1 vs. c (whether to amend the bill)
3. x vs. y (whether to amend the substitute bill)
4. winner at 3 vs. z (whether to replace the [perfected] substitute by the substitute to the substitute)
5. winner at 2 vs. winner at 4 (whether to replace the [perfected] bill by the surviving substitute)
6. winner at 5 vs. q (whether to pass the surviving motion)

Suppose a three-member committee holds the following preference orders:

Member 1: $a\ c\ z\ b\ y\ x\ q$
Member 2: $b\ y\ c\ x\ a\ z\ q$
Member 3: $x\ z\ a\ y\ c\ b\ q$

The majority-preference relation is as follows, where, aside from the exceptions noted by arrows, the left-to-right order of

Figure 12. Agenda for Example 3



the motions corresponds to the majority preference:

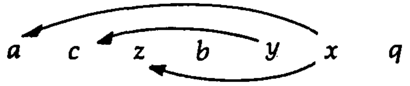
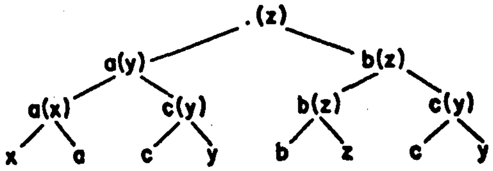


Figure 12 portrays the agenda and, in parentheses, shows the strategic choice from each subtree (to save space we omit q from the tree since it is defeated by everything else). So $STRAT(T) = z$. But $z \notin U(\alpha)$: a covers z because a beats z and also beats the three alternatives (b, y, and q) that z beats.

To see why a covered alternative is chosen, consider the eight subtrees, $T1$–$T8$, in Figure 12. Each is equivalent to a mini-amendment agenda (e.g., in $T1$, x is paired with z, then the winner with a), so from result (3), the strategic choice from each is uncovered in that subtree. Since no information about outcomes is lost if we replace a subtree by its strategic choice, we redraw the agenda in Figure 13 to extend each branch down just to where subtrees become amendment agendas (for $T5$ we substitute b for z since z beats b and will beat b in the corresponding comparison with the strategic choice from $T6$).

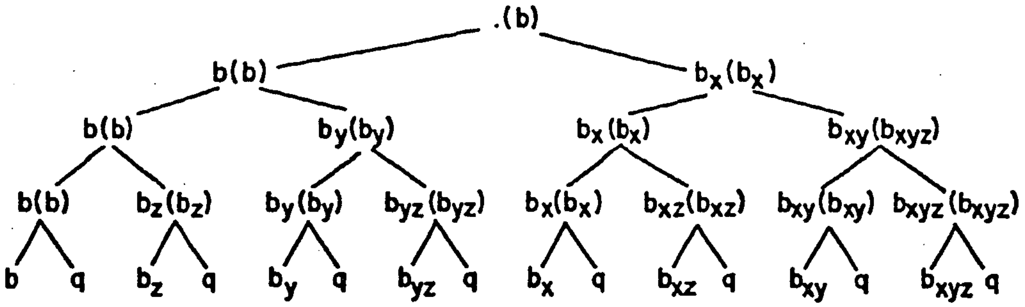
Figure 13. Reduced Agenda



This tree is almost an amendment agenda: after a and b are paired, the winner is paired with c, and the winner in that context with y. The important exceptions are that if a wins in the first two rounds, x and a are paired (rather than a and y), whereas if b wins in those rounds, b is paired with z (rather than y). Thus, x can defeat a and then lose to y without ever being compared to z. The discontinuities permit this asymmetry in the treatment of z and a and account for the survival of a covered alternative. Although the original tree is symmetric, the strategically equivalent reduced tree, owing to the discontinuity, is not symmetric.

Example 4. It would be a mistake to infer from the preceding examples that symmetry or discontinuity is essential to agendas in which strategic voting leads outside the uncovered set. Consider an agenda involving three compatible first-order amendments, x, y, and z. Suppose P

Figure 14. Agenda for Example 4



orders the nine alternative outcomes, from left to right, thus:

$$b_{xyz} \; b_{xy} \; b \; b_x \; b_y \; b_z \; b_{yz} \; b_{xz} \; q$$

with the exception that $b_x \, P \, b_{xyz}$. Then b_{xy} covers b. Nevertheless, the appropriate agenda, depicted in Figure 14 with strategic equivalents in parentheses, yields b as the final outcome. Although not symmetric, this agenda is continuous, uniform, and nonrepetitive.

These examples, then, establish the amendment-agenda assumption as essential to Results (1)–(4). They establish the same for the three other propositions that we discuss in the previous. First, *strategic choices need not be Pareto-efficient.* In Example 3, the strategic choice, z, is covered but Pareto efficient. Now switch a and z in Member 3's preference ordering. Then P remains the same, but z becomes Pareto inefficient.

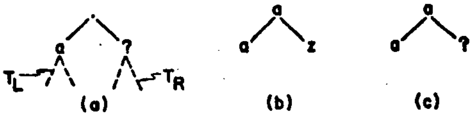
Second, *the first alternative on an agenda can cover the final strategic choice.* In Example 3, a is voted on first, and a covers z, yet z is the outcome under strategic voting.

Finally, *not every agenda can be reduced to a $\leqslant$ two-step agenda with the same first alternative and the same strategic choice.* Looking again at Example 3, notice that every one- or two-stage agenda T that begins with a and yields z as the strategic choice must have the form shown in Figure 15a. Since $z = STRAT(T)$, z must also equal $STRAT(T_L)$ or $STRAT(T_R)$. If $z = STRAT(T_L)$, then T_L must have the form shown in Figure 15b since, with two or fewer steps, this must be the last step and only a and z can be paired. But a beats z, so $z \neq STRAT(T_L)$. Suppose, then, that $z = STRAT(T_R)$. Then T_L must have the form shown in Figure 15c, where ? is an alternative that beats a but loses to z (so that z survives as the final outcome). But the only alternative that defeats a is x, which z does not defeat. Thus, the agenda in Figure 14 cannot be reduced to one or two stages with alternative a entering the first stage and z being the final outcome.

Theorems

The preceding examples demonstrate that none of the earlier results specifying limits on a hypothetical agenda setter's power under strategic voting applies when that agenda setter's strategy set contains all agendas permitted in real legis-

Figure 15. Two-Step Reduction of Example 3



latures, such as the U.S. Congress. The results we prove in this section show how wide the true limits are, both for strategic and sincere voting. When compared with the theorems reviewed earlier, these results reveal exactly how special a case the amendment agenda really is. We begin with an unsurprising result:

THEOREM 3. *If T is any agenda on α, then $STRAT(T) \in G(\alpha)$.*

Because $U(\alpha)$ and $B(\alpha)$ are subsets of $G(\alpha)$, this theorem does not exclude the possibility that an agenda setter's power is restricted by strategic voting to these particular subsets of $G(\alpha)$. Strategic voting does restrict a setter's power, but not in the way we might think. First, consider the "kitchen sink" set:

$$K(\alpha) = \{x \in \alpha \mid \alpha = \{x\} \text{ or } x\,P\,y \text{ for some } y \in \alpha\}.$$

Thus, x is in $K(\alpha)$ so long as α consists of x alone or x defeats something or other in α. Clearly, $G(\alpha) \subseteq K(\alpha)$, and $K(\alpha)$ comprises all but at most one member of α. Not surprisingly:

THEOREM 4. *If T is an agenda on α, $SIN(T) \in K(\alpha)$.*

Like Theorem 3, this result places weak bounds on final outcomes. What is interesting, however, is that *these bounds are the strongest there are:* the G and K sets are the exact limits of an agenda setter's power under strategic and sincere voting, respectively.

To see what we mean, consider a sequential-elimination agenda, the general form of which is that of Figure 4, but with any number of nodes. Our next theorem says that *any* outcome in $K(\alpha)$—which is all of α less at most one member—can be the sincere choice from some nonrepetitive sequential-elimination agenda, hence from some nonrepetitive continuous agenda:

THEOREM 5. *If $x \in K(\alpha)$, then for some nonrepetitive sequential-elimination agenda T on α, $x = SIN(T)$.*

When the set of feasible agendas is restricted to the amendment agendas, or even the symmetric ones, sincere voting cannot lead outside the G-set, but as soon as the feasible agendas are allowed to include even nonrepetitive sequential-elimination agendas (hence, more generally, continuous agendas), sincere voting can lead practically anywhere.

We can now see one weak limit, however, that strategic voting places on an agenda setter's power: although large, $G(\alpha)$ is often a proper subset of $K(\alpha)$. But because $G(\alpha)$ might be large and some of its elements of Pareto inefficient, we want to know whether strategic voting places more severe limits on an agenda setter's power. The next five results show that tighter limits do not exist except in special cases.

THEOREM 6. *If $x \in G(\alpha)$, there exists a nonrepetitive sequential-elimination agenda T on α such that $STRAT(T) = x$.* (Moulin 1985)

Figure 16 summarizes the Theorems 3–6.

If we drop the requirement of nonrepetitiveness in Theorem 6, we can ensure that T begins with any given alternative and yields any other given alternative as the strategic choice:

THEOREM 7. *If $x \in G(\alpha)$, then there exists a sequential-elimination agenda T on α such that y is the first alternative in T and $x = STRAT(T)$.*

McKelvey's theorem about agendas leading anywhere under sincere voting can be restated for strategic voting if, instead of amendment agendas, we use sequential-elimination agendas:

THEOREM 8: *For the "standard spatial model," if X is the issue space, if the majority-preference relation M is a* *subrelation of P, and if no dominant point in X exists, then for any two points $x, y \in X$, there is a nonrepetitive sequential-elimination agenda T on some finite subset of X such that (1) x is the first alternative in T, (2) $y = STRAT(T)$, and (3) $STRAT(T'_L)$ M $STRAT(T'_R)$ or conversely for every $>$ 2-node subtree T' of T.*

Since P is connected but M is not, we might think of P as the result of extending M by the addition of some tie-breaking rule. Clause (3) says that no M-ties actually arise in T. The essential point of this theorem, though, is that it is not strategic voting per se that limits choices to the uncovered set or the Banks set but the assumption that an agenda setter must choose agendas of the amendment type.

Theorem 6 tells us that if all voters are strategic and if all binary agendas are admissable, then there exists some binary agenda on α that will choose any outcome in $G(\alpha)$, whereas from the results reviewed in the Choice Sets and Theorems section, we know that if agendas are limited to the amendment type, then only outcomes in $B(\alpha) \subseteq G(\alpha)$ can prevail. It is interesting, however, to consider separately the class of symmetric agendas since one-period agendas often are required to be symmetric (e.g., by House Rule XIV). Of course, our examples from the previous section tell us that such agendas can lead outside the Banks set (Example 2), outside the uncovered set (Examples 3 and 4), and outside the Pareto set (Example 3 with modified preferences for Member 3). Often, though, the rule assigned to a bill in the House limits the number of alternatives that can be voted on, whence the importance of the following theorem:

THEOREM 9. *If α has 5 or fewer members, then for any symmetric agenda T on α, $STRAT(T) \in U(\alpha)$.*

Thanks to Example 3, the five-alternative limit cannot be raised. Hence, earlier results do extend to certain nonamendment agendas if the number of alternatives is severely constrained: even under strategic voting, the power of an agenda setter is no greater with a strategy set comprising all symmetric agendas than with a strategy set limited to amendment agendas if the alternatives that can be included in the agenda are five or fewer. But what if they exceed five? Can just any outcome in $G(\alpha)$ be achieved with a symmetric agenda, as it can with a sequential-elimination agenda, or are final outcomes limited to a set lying "between" $U(\alpha)$ and $G(\alpha)$? Here is a partial answer:

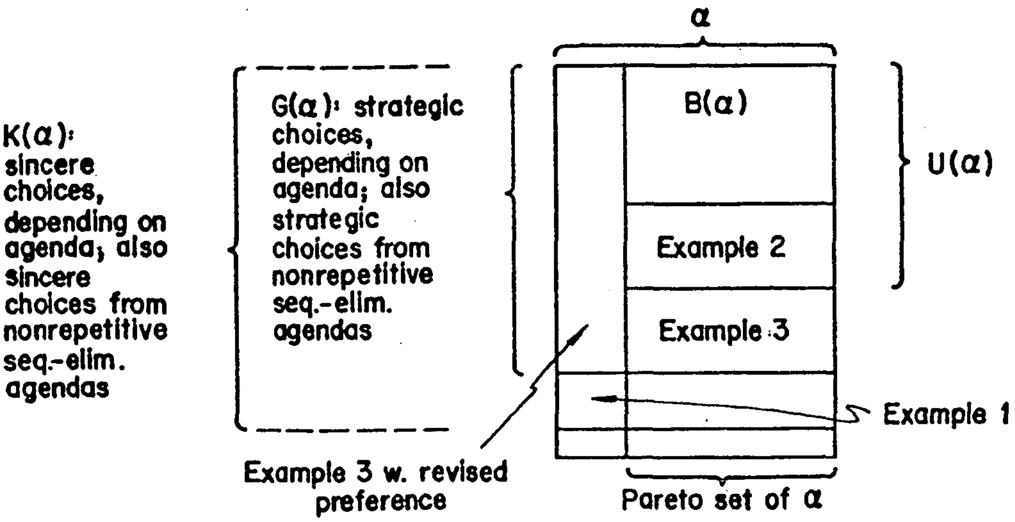
THEOREM 10. *Suppose T is a symmetric agenda on α, α has at least 4 members, and x is covered in α by all but 2 other members of α. Then $x \neq STRAT(T)$.*

Notice that if $|\alpha| \geqslant 4$ and if $x \in G(\alpha)$, then there must be at least two other members of α that do not cover x, whereas everything else in α can cover x (see the construction used to prove the theorem). Given the assumptions of Theorem 10, although $x \in G(\alpha)$, x cannot be chosen from any symmetric agenda if voters are strategic. Thus, it appears that the set of reachable outcomes when only symmetric agendas are feasible lies somewhere between what is reachable if only amendment agendas are feasible and what is reachable if all binary agendas are feasible.

Conclusions

It is tempting to interpret our analysis as supporting the idea that political outcomes are at the mercy of elites who control agendas. Certainly, our analysis supports the proposition that strategic voting is not always effective for thwarting the intent of an unconstrained agenda setter, but it is naive to interpret our results as implying a reemergence of the power of agenda setters, hypothetical or real. For

Figure 16. Summary of General Agenda Results


α
K(α): sincere choices, depending on agenda; also sincere choices from nonrepetitive seq.-elim. agendas
G(α): strategic choices, depending on agenda; also strategic choices from nonrepetitive seq.-elim. agendas
B(α)
Example 2
Example 3
Example 1
U(α)
Example 3 w. revised preference
Pareto set of α

one thing, the set of feasible legislative agendas, even with a handful of motions, may be large. And Hammond's (1986) adaptation of agendas to the study of hierarchical decision processes suggests that the set of feasible organizational agendas may be larger still. Hence, power may lie more with those who are best able to ascertain the implications of particular agendas than with those who control procedures and agenda form.

Second, in addition to the formal constraints imposed by such measures as Rule XIV, setters are also subject to those who would upset their calculations by the introduction of new issues and dimensions (Riker 1982). And as the experiences of Plott and Levine (1976) attest, even those unschooled in agenda manipulation soon learn the importance of agenda control and seek the means to thwart that control. Third, our analysis assumes that all preferences are common knowledge. But as Ordeshook and Palfrey (1986) demonstrate, outcomes can be radically different with incomplete information. Although their analysis is limited to amendment agendas, it shows how agendas reveal different things about preferences, suggesting that a setter can manipulate the agenda to reveal preferences in particular ways.

We also should consider the ways in which motions are introduced for consideration. With the exception of Theorem 6, which concerns spatial majority-rule games, our analysis focuses on the manipulation of outcomes by deciding how some fixed set of alternatives is considered and disposed of. But agenda power has two sources: the ability to choose a procedure for selecting from some fixed set of alternatives and the ability to choose that set. And we suspect that agenda power, if it exists at all in Congress, has more to do with the second source than the first. The labeling of alternatives as *the bill, an amendment,* or *a substitute amendment* is an important strategic process, but once all motions are made and the labels attached, there may be little room for strategic maneuver.

The relevance of agendas to the study of final outcomes, then, may depend on the rules that determine the alternatives that a legislature considers. Here, however, the possible endogenicity of the process complicates matters. Some progress has been made in the study of endogenous agenda formation (McKelvey 1986), but again under the assumption that all agendas are of the amendment type. The variety of congressional agendas and the limits that might otherwise be placed on the types of agendas that a legislature can use raise new research possibilities. The labeling of motions may be an important strategy available to members because the decision about how to label a motion has more profound consequences than earlier research indicates. The sole strategic consideration in an amendment agenda is the order of the alternatives. But if, for example, two proposed amendments to a committee report are both moved and recognized as perfecting amendments rather than as a perfecting amendment and a substitute bill, the prescribed agenda must be nonsymmetric (as in Figure 2).

Hence, instead of proclaiming the preeminence of the agenda setter—hypothetical or otherwise—our analysis warns us that our subject concerns games in which participants' strategies include more than simply deciding the order in which to present motions or whether to vote sincerely on the current ballot. We are only beginning to identify and understand those strategies, but this essay sheds some light on the potential consequences of part of the strategy space of those who participate in the institutions of social choice.

Appendix

Proof of Theorem 1. Because symmetric agendas are obviously complete and uniform, we prove only that they are nonrepetitive. If T on α is symmetric and repetitive, then some subtree of T is equivalent to a tree T' in which some $x \in \alpha$ occupies the top node of T'_L, some $y \in \alpha$ occupies the top node of T'_R, but x occurs in T'_R, say at node n. By symmetry, T'_L is the same as T'_R but for the substitution of x for y. So x must occur at node n in T'_L as well. But because we can get T'_R from T'_L by substituting y for x, y must occur at node n in T'_R, which is impossible since x occurs there. Q.E.D.

Proof of Theorem 2. We first prove, by induction on n, the lemma that if $T = A(x_1, \ldots, x_n)$ is an amendment agenda, then $(x_1, \ldots, x_n)$ contains no repetitions. This is trivially true if $n = 1$. Otherwise, $T_L = A(x_1, x_3, \ldots, x_n)$ and $T_R = A(x_2, x_3, \ldots, x_n)$ are agendas. By inductive hypothesis, $(x_1, x_3, \ldots, x_n)$ and $(x_2, x_3, \ldots, x_n)$ contain no repetitions, so if $(x_1, \ldots, x_n)$ contained a repetition, we would have $x_1 = x_2$, in which case x_1 $(= x_2)$ would occupy the top nodes of both T_L and T_R, contrary to the definition of an agenda.

We prove the theorem itself by induction on the number of nodes of T, which we assume exceeds 1 (else the theorem is trivially true). Let T be an amendment agenda, so that T is equivalent to $T' = A(x_1, \ldots, x_n)$ for some $x_1, \ldots, x_n$ $(n \geqslant 2)$. Then $T'_L = A(x_1, x_3, \ldots, x_n)$ and $T'_R = A(x_2, x_3, \ldots, x_n)$, which are continuous and symmetric by inductive hypothesis. Therefore, since x_1 occupies the top node of T' and T'_L while x_2 occupies the top node of T'_R, T' is continuous. But since, by the lemma, $(x_1, x_3, \ldots, x_n)$ and $(x_2, x_3, \ldots, x_n)$ contain no repetitions, T'_L is the result of substituting x_1 for x_2 in T'_R, while T'_R is the result of substituting x_2 for x_1 in T'_L. Hence, owing to the symmetry of T'_L and T'_R, T' is symmetric.

Conversely, suppose T is continuous and symmetric. Then T is equivalent to an agenda T' in which, for some x and y, x occupies the top nodes of T' and T'_L while y occupies the top node of T'_R. By inductive hypothesis, T'_L is equivalent to $A(x, x_1, \ldots, x_n)$ and T'_R to $A(y, y_1, \ldots, y_m)$ for some $x_1, \ldots, x_n$

and $y_1, \ldots, y_m$ ($n \geqslant 0$, $m \geqslant 0$). But by symmetry, $A(y,y_1,\ldots,y_m)$ is the result of substituting y for x in $A(x,x_1,\ldots,x_n)$, and by the lemma, $(y,y_1,\ldots,y_m)$ and $(x,x_1,\ldots,x_n)$ have no repetitions. So $A(y,y_1,\ldots,y_m) = A(x,x_1,\ldots,x_n)$. Hence, T' is equivalent to $A(x,y,x_1,\ldots,x_n)$. Q.E.D.

Proof of Theorem 3. By induction on the number of nodes of T. Trivial if T has one node. Otherwise, let α_L be the set of alternatives in T_L, let α_R be the set of alternatives in T_R, and let $\alpha = \alpha_L \cup \alpha_R$. By inductive hypothesis,

$$STRAT(T_L) \in G(\alpha_L) \tag{A1}$$

$$STRAT(T_R) \in G(\alpha_R) \tag{A2}$$

Without loss of generality, suppose that

$$STRAT(T) = STRAT(T_L) \tag{A3}$$

From the definition of $G(\alpha)$, everything in $G(\alpha)$ bears P to everything in $\alpha - G(\alpha)$, while everything in $G(\alpha_L)$ bears P to everything in $\alpha_L - G(\alpha_L)$. So if there existed a $w \in G(\alpha_L) - G(\alpha)$ and a $v \in G(\alpha) \cap \alpha_L$, we would have both wPv and vPw, contrary to the asymmetry of P. Hence, either $G(\alpha_L) - G(\alpha)$ or $G(\alpha) \cap \alpha_L$ is empty. That is, either $G(\alpha_L) \subseteq G(\alpha)$ or $G(\alpha) \cap \alpha_L = \emptyset$. Similarly, either $G(\alpha_R) \subseteq G(\alpha)$ or $G(\alpha) \cap \alpha_R = \emptyset$. Consequently, if $G(\alpha_L) \not\subseteq G(\alpha)$, then $G(\alpha) \cap \alpha_L = \emptyset$, so that $G(\alpha) \cap \alpha_R \neq \emptyset$ (since otherwise $G(\alpha) = G(\alpha_L \cup \alpha_R)$ would be empty), whence $G(\alpha_R) \subseteq G(\alpha)$, and thus, by (A1) and (A2), $STRAT(T_R) \in G(\alpha)$ (because $G(\alpha) \cap \alpha_L = \emptyset$), but $STRAT(T_L) \notin G(\alpha)$, which implies that $STRAT(T_R)\ P\ STRAT(T_L)$, contrary to (A3). Hence, $G(\alpha_L) \subseteq G(\alpha)$ after all, and so by (1) and (3), $STRAT(T) \in G(\alpha)$. Q.E.D.

Proof of Theorem 4. By induction on $|\alpha|$. Trivial if $|\alpha| = 1$. Otherwise, let y occupy the top node of T_L and z the top node of T_R. Without loss of generality, suppose $y\ P\ z$. Then $SIN(T) = SIN(T_L)$. If T_L consists just of y, then $SIN(T) = SIN(T_L) = y\ P\ z$, so $SIN(T) \in K(\alpha)$. On the other hand, if T_L has more than one node, then by inductive hypothesis, $SIN(T) = SIN(T_L)\ P\ w$ for some $w \in \alpha$, and so $SIN(T) \in K(\alpha)$. Q.E.D.

Our next results require a formal definition of sequential-elimination agendas.

A *sequential-elimination agenda* is an agenda that is equivalent to $S(x_1, \ldots, x_n)$ for some $x_1, \ldots, x_n$, where $S(x_1, \ldots, x_n)$ is defined thus: $S(x_1)$ is the 1-node tree occupied by x_1; $S(x_1, \ldots, x_n)$ is the binary tree T in which x_n occupies the top node, T_R is the 1-node tree occupied by x_1, and $T_L = S(x_2, \ldots, x_n)$.

Proof of Theorem 5. Trivial if $\alpha = \{x\}$. Otherwise, there exists a $y \in \alpha$ for which $x\ P\ y$. So there exist $y_1, \ldots, y_n$ such that (1) $\alpha - \{x\} = \{y_1, \ldots, y_n\}$, (2) $y_i \neq y_j$ unless $i = j$, and (3) $y_1 = y$. Then for the nonrepetitive sequential-elimination agenda $T = S(x, y_1, \ldots, y_n)$, $SIN(T) = SIN(T_R) = x$, because $x\ P\ y_1 = y$. Q.E.D.

Two definitions and a lemma. Before proceeding to the remaining theorems we need the following:

DEFINITION. *A* path *on α is a vector $(x_1, x_2, \ldots, x_n)$ such that $\alpha = \{x_1, x_2, \ldots, x_n\}$ and for each $x_i \neq x_1$, $x_j\ P\ x_i$ for some $j < i$. It is* nonrepetitive *if $x_i \neq x_j$ whenever $i \neq j$.*

LEMMA. If $\mathbf{x} = (x_1, x_2, \ldots, x_n)$ is a path on α, then there exists a sequential-elimination agenda T on α such that (1) $STRAT(T) = x_1$, (2) T begins with x_n, and (3) T is nonrepetitive if $\mathbf{x}$ is nonrepetitive.

Proof. Trivial if $n = 1$. Otherwise, let $T = S(x_1, \ldots, x_n)$. Then T begins with x_n and is nonrepetitive if $\mathbf{x}$ is. For $i = 1, 2, \ldots, n-1$, let T_i be the subtree of T in which the first vote is between x_n and x_i. Then it suffices to show, by induction on $n-i$, that for every $i < n$, either $STRAT(T_i) = x_i$ (so that, in particular, $STRAT(T_1) = x_1$), or $STRAT(T_i) = x_1$, or $x_j\ P\ STRAT(T_i)$ for some $j = 1,2, \ldots, i-1$. If $x_n\ P\ x_{n-1}$, then $STRAT(T_{n-1}) = x_n$ and, since $\mathbf{x}$ is a path, either $n = 1$ or $x_j\ P\ STRAT(T_{n-1}) = x_n$ for some $j = 1,2, \ldots, n-2$. Otherwise, $x_{n-1}\ P\ x_n$, so $STRAT(T_{n-1}) = x_{n-1}$. Now suppose that $i < n-1$. If $x_i\ P\ STRAT(T_{i-1})$, then $x_o = STRAT(T_i)$. On the other hand, suppose $STRAT(T_{i+1})\ P\ x_i$. Then $STRAT(T_i) = STRAT(T_{i+1})$. But by inductive hypothesis, either $STRAT(T_{i+1}) = x_1$, or $x_j\ P\ STRAT(T_{i+1})$ for some $j < i+1$, or else $STRAT(T_{i+1}) = x_{i+1}$, in which case, since $\mathbf{x}$ is a path, we again have $x_{i+1} = x_1$ or $x_j\ P\ x_{i+1} = STRAT(T_{i+1})$ for some $j < i+1$. Hence, since not $x_i\ P\ STRAT(T_{i+1})$, there must exist a $j < i$ such that $x_j\ P\ STRAT(T_{i+1}) = STRAT(T_i)$ unless $STRAT(T_i) = STRAT(T_{i+1}) = x_1$.Q.E.D.

Proof of Theorem 6. Trivial if $\alpha = \{x\}$. Otherwise, if $\alpha - G(\alpha) \neq \emptyset$, let $\alpha - G(\alpha) = \{y_1, \ldots, y_k\}$ where $y_i \neq y_j$ whenever $j \neq i$. We have two cases:

Case 1. $G(\alpha) = \{x\}$. Then $x\ P\ y_i$ for all i. So $(x, y_1, \ldots, y_k)$ is a nonrepetitive path on α, and the theorem follows from the lemma.

Case 2. $G(\alpha)$ is a dominant cycle in α. Then there exist $x_1, \ldots, x_m$ such that (1) $G(\alpha) = \{x_1, \ldots, x_m\}$; (2) $x_1 = x$; (3) $x_1\ P\ x_2\ P \cdots P\ x_m\ P\ x_1$; and (4) $x_i\ P\ y_j$, $j = 1,2, \ldots, k$; $i = 1,2, \ldots, m$. Let $(x'_1, \ldots, x'_r)$ be the result of deleting from $(x_1, \ldots, x_n)$ all but the first occurrence of each x_i that occurs more than once. Then $x'_1 = x$, and $(x'_1, \ldots, x'_r, y_1, \ldots, y_k)$ is a nonrepetitive path on α, whence the theorem follows by the lemma. Q.E.D.

Proof of Theorem 7. Trivial if α consists of a single alternative. Otherwise, if $\alpha - G(\alpha) \neq \emptyset$, let $\alpha - G(\alpha) = \{z_1, \ldots, z_k\}$. We now have the same two cases to consider as before.

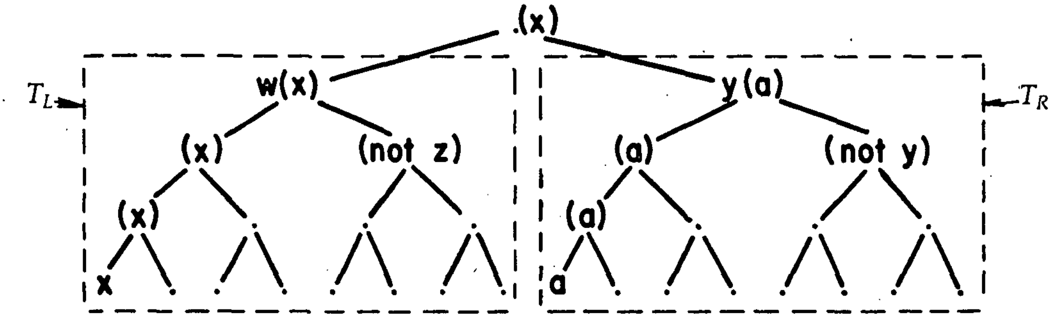
Case 1. $G(\alpha) = \{x\}$. Then $x\ P\ z_i$ for all i, and either $x = y$ or $x\ P\ y$. So $(x, z_1, \ldots, z_k, y)$ is a path, and the theorem follows by the lemma.

Case 2. $G(\alpha)$ is a dominant cycle in α. Then the same four conditions hold as in the proof of Theorem 6 except that the last condition (4) reads: $x_i\ P\ z_j$ for all i and j. Consequently, either $y = x = x_1$ or $y = x_i$ for some $i > 1$ or $y = z_j$ for some j. So $(x_1, \ldots, x_m, z_1, \ldots, z_k, y)$ is a path in which $x_1 = x$, and the theorem follows from the lemma. Q.E.D.

Proof of Theorem 8. There exist $x_1, \ldots, x_n$ such that (1) $x_1 = x$, (2) $x_n = y$, and (3) $x_1\ P\ x_2\ P \cdots P\ x_n$, where P is a connected extension of the majority-preference relation (McKelvey 1979). Since $(x_1, \ldots, x_n)$ is a path, the theorem follows from the lemma. Q.E.D.

Proof of Theorem 9. Since q is not critical to Example 3 in Section 5, as few as six alternatives are required to generate a symmetric agenda that leads outside of $U(\alpha)$. To see that we cannot construct an example with fewer alternatives, let some alternative x be covered by y (so $y\ P\ x$). Then for x to prevail, we must eliminate y, say with z. But if $z\ P\ y$, then $z\ P\ x$ (since anything that defeats y defeats x), so z also must be eliminated by an alternative, say w, for which $x\ P\ w$. Otherwise, if $\alpha = \{x,y,z\}$ or if $\alpha = \{x,y,z,w\}$ with $w\ P\ x$, then $x \notin G(\alpha)$ and x cannot prevail. So we must have $\alpha = \{x,y,z,w\}$ with $x\ P\ w$ (and since y covers x, with $y\ P\ w$). But even this is not enough. For x to be the outcome, at least one principal subtree, say T_L, must have $x = STRAT(T_L)$, and x or w must equal $STRAT(T_R)$. If T is symmetric, then T_L must look like an amendment agenda over three members of α, and the only such agenda that yields x is "z vs. x, the winner against w." So the first ballot of T involves y, which enters T_R, and T_R yields w only if it is the agenda "y vs. w, the winner against z." But if the first node of T_L pits z against x and if the first node of T_R pits y against w, then T is

Figure A-1. Agenda Beginning with w and y



not symmetric. This shows that if x prevails, then α cannot have four or fewer elements.

Now let $\alpha = \{a,x,y,z,w\}$ and, as before, let $x = STRAT(T_L)$ and let a or w equal $STRAT(T_R)$. Since $STRAT(T_L) = x$, then, from the previous argument that no covered alternative can prevail in any four-alternative symmetric agenda, y cannot enter T_L. Thus, y must be voted on in the first ballot of T and enter T_R. But x cannot be paired with y in this first ballot. Since y must be defeated by something in T_R for x to prevail in T, pairing x and y first means that T_L is identical to T_R except for the interchange of x and y, in which case, whatever defeats y also defeats x. Hence, we have three cases, depending on what, besides y, is voted on in the first ballot.

Case 1. If the first ballot pairs y and w, then the agenda must look like the one shown in Figure A-1, where the outcomes in parentheses denote strategic choices. Alternative a must be the strategic choice of T_R since it is the only alternative in this subtree that does not necessarily defeat x. But if $x\ P\ a$, then $y\ P\ a$, in which case we must have $a\ P\ z$, else $a \notin G(\{x,y,z,a\})$ and a would not equal $STRAT(T_R)$ as assumed. If, without loss of generality, $a = STRAT(T_{R_L})$ as indicated, then T_{R_L} must correspond to the agenda "y vs. a, winner against z" or to "x vs. a, winner against z." But if z enters the agenda only in the last ballot, it must do so also in T_L, owing to the symmetry of T, and since $z\ P\ x$, x cannot rise in T_L to become $STRAT(T_L)$.

Case 2. If the first ballot pairs a and y, then the agenda looks like the preceding picture except that w and a reverse roles. But by the same argument, $w = STRAT(T_R)$ only if the left subtree of T_R is the agenda "y vs. w, the winner against z" or "x vs. w, the winner against z," so again z enters the agenda only in the last ballot and $x \neq STRAT(T_L)$.

Case 3. If the first ballot pairs z and y, then we must have $w = STRAT(T_R)$, since, to ensure that $y \neq STRAT(T_R)$, we must have $a\ P\ y$ and hence $a\ P\ x$. Also, to ensure that $w \in G(\{a,x,y,x\})$, we must have $w\ P\ a$. Substituting w for a in T_R in the previous figure, $w = STRAT(T_R)$ only if the left subtree of T_R is the agenda "x vs. w, the winner against a" or "y vs. w, the winner against a." But now, as in Case 1, the symmetry of T requires that a also enter the agenda in the final ballot of T_L, and since $a\ P\ x$, $x \neq STRAT(T_L)$.

Q.E.D.

Proof of Theorem 10. By induction on $|\alpha|$. Assume that $x = STRAT(T)$. Then $x \in G(\alpha)$ by Theorem 3. Since x is covered, $G(\alpha)$ is a cycle, so $x\ P\ y\ P\ w$ for some y, $w \in G(\alpha)$. Let $\{z_1, \ldots, z_n\} = \alpha - \{x,y,w\}$. Since $x\ P\ y$, y does not cover x, and since $y \; P \; w$ and anything that covers x bears P to y, w does not cover x. So $z_1, \ldots, z_n$ cover x by hypothesis of the theorem. Hence, $z_i \; P \; x$ and $z_i \; P \; y$ for all i, and thus, since $G(\alpha)$ is a dominant cycle in α, w bears P to some z_i, say $w \; P \; z_1$. Because $x = STRAT(T)$, x equals $STRAT(T_L)$ or $STRAT(T_R)$; say $x = STRAT(T_L)$. By Theorem 3, $x \in G(\alpha_L)$, where α_L is the set of alternatives in T_L. By the symmetry of T, α_L lacks just one member of α. Hence, $y,w \in \alpha_L$ (else $x \notin G(\alpha)$). So if any z_i belonged to α_L, we would have $x \neq STRAT(T_L)$ by inductive hypothesis. Thus, no z_i belongs to α_L. Consequently, $n = 1$ and $\alpha = \{x,y,w,z_1\}$. Since $|\alpha| = 5$, the theorem follows from Theorem 9. Q.E.D.

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PRISONERS' FINANCIAL DILEMMAS: A CONSOCIATIONAL FUTURE FOR LEBANON?

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Lebanon's banking system is viewed as an "international regime" that held the country together in the face of all pervasive warfare. The system illustrates the underlying logic of such regimes, in that the banks' credit policies operationalized the rational choice model of an iterated prisoner's dilemma game. And just as international regimes are supposed to reflect the interests of state actors, analysis of the banks' balance sheets and income statements reveals changes in capital structure that reflect changes in Lebanon's political balance. While sustaining elite cohesion, the banks serviced political clienteles and might, if a political settlement were reached, support the restructuring of a consociational system more in line with Lebanon's demographic balance. If, on the other hand, no settlement is reached, the poor country's suicide may highlight certain aspects of international reality that seem recalcitrant to the theory of international regimes.

Whereas Plato sought ethical principles by "writing large" the travail of the human soul into a well-ordered political macrocosm, contemporary students of international relations write small. They derive theories of cooperation from microeconomics and prisoners' dilemmas. In a world without enforceable centralized authority or the proxy of a hegemonic state, the Hobbesian sovereign gives way to intermediaries who, like used car dealers, are supposed to prevent "market failure" (Akerlof 1970; Keohane 1984). This fresh, albeit shadowy, "grey in grey" (Hegel 1953, 13) perspective on international regimes may also shed light on domestic situations. In Lebanon the level of conflict possibly exceeds even Hobbes's visions of brutality and nastiness. In this paper I will view Lebanon's banking system as the principal "regime" that supported the country's political elite as well as its economy in the facce of uncivil, multidimensional warfare.

The new perspective of international cooperation rests squarely on concepts of transaction and information costs borrowed from financial rather than political theory. As is well known, however, "economics proceeds in a context framed by law and order, established by politics. Economic life continues in wartime and in periods of revolution and anarchy, but under conditions that must be regarded as pathological" (Kindleberger 1970, 15). How then can financial concepts apply to a world of anarchy, in which property rights and contracts are unenforceable? Lebanon offers a practical illustration in which to ground these concepts and recognize their limits.

Since 1975 the country has replicated in microcosm the condition of international anarchy posited by the structural realists, yet its "pathological" economy survived and in some sectors prospered until the Israeli invasion of 1982. Its banking system continued for a time to finance industry, illustrating the new logic of international cooperation and its limits. The bankers cooperated by rolling over their shares of loans, unwittingly operationalizing the rational choice model of an iterated Prisoner's Dilemma game. They also unintentionally adapted to shifts in political power. The banking regime, in fact, supported a potentially functioning polity. In this light, financial intermediation can be understood as the elite adhesive required to make consociational democracy work. It will be shown that the banks' capital structure changed —and could change further—in ways that better reflect the country's changing political and demographic balance.

The argument of this paper will be developed in four stages. First, the theoretical rationale for international regimes is translated back into its financial nexus. A theory of financial intermediation can shed light on the Lebanese banking system and explain why a "regime" was needed. Secondly, the system will illustrate the functioning of the regime under conditions of anarchy. Thirdly, the conditions for its demise or transformation will be analyzed in light of the Prisoner's Dilemma. And, finally, Lebanon's banking regime will be related to conventional political analyses of the country's past and its potential futures.

International Regimes and Theories of Financial Intermediation

In systems lacking centralized enforcement, *regimes* can be generally defined as "sets of implicit or explicit principles, norms, rules, and decision-making procedures around which the actors' expectations converge" (Krasner 1983, 2). Keohane (1984) compares them to credit markets (p. 129) and explains in his seminal work why actors' expectations might continue to converge around them even when the balance of power changes: regimes can be understood "as information-producing and transaction cost-reducing entities rather than as quasi-governmental rule-makers" (p. 101). They are supposed to enable the actors—typically state actors—to stave off market failure in some sense. The concepts, however, were extracted from the world of finance, not politics.

The spectre of market failure may be easier to locate in purely economic activities, however political they may also be, than in conventional power relationships among states. Most intended regimes, of course, comprise interrelationships between state actors, but the actors need not be states. The international banking system may display a regime, for instance, insofar as banking operations across national lines escape the jurisdictions of the banks' parent countries. To the extent that relationships between Less Developed Countries (LDC) debtors and creditors remain viable, it makes sense to speak of an international banking regime of nonstate actors (Lipson 1985). Our theoretical problem is to discover the sources of Pareto-deficient equilibria—or market failure—that a regime of financial intermediation might remedy.

The financial literature offers a variety of models to explain the underlying rationale for financial intermediation in advanced capitalist systems. The multiplicity of explanations is due, of course, to the variety of possible market imperfections financial theorists may model (Campbell and Kracaw 1980; Chan 1983). If the present analysis were intended to offer historical clues about the rise of banking systems, an eclectic approach would be in order (Kindelberger 1983). Transaction costs are obviously fundamental to explanations of financial systems in trading countries like Lebanon. So, too, a developmental perspective would stress transaction costs and asset transformation because the focus is on mobilizing savings for industrial investment (Abdi 1977). But most of the market failure literature is really about information (Chan 1983), and some of it analyzes conditions in advanced capitalist societies under which banks may substitute for capital markets. This analysis is the most useful for Lebanon because it was the banks' medium and long-term lending, not their principal business of trade financing, that was the greatest anomaly in the wartime situation.

The source of the problem is quality uncertainty offered in markets characterized by informational asymmetries (Akerlof 1970). Without reliable used car dealers, for example, people will only offer lemons for sale because buyers will be unable to distinguish them from cream puffs. Buyers will not have adequate incentive to acquire the information needed to make the necessary distinctions, nor will anyone else, given the public goods nature of information, unless perhaps a used car dealer can trade on (lower-cost) information and acquire a good reputation. Financial theorists have developed this line of argument into a convincing rationale for financial intermediation in societies where contracts can be enforced. It is useful to summarize the argument and its underlying assumptions before extending it to other societies where contracts cannot be enforced.

Like Akerlof's used cars, investment projects vary in quality. Assume that each entrepreneur knows the value of the project but is unable reliably to communicate it to potential investors. In the investment market, bad projects will drive out good ones, and potential investors will be unwilling to invest their funds, assuming that alternative placements (e.g., in other markets, outside Lebanon) are available. As Leland and Pyle (1977, 383) point out,

> two problems . . . hamper firms which might try to sell information directly to investors. The first is the appropriability of returns by the firm—the well known 'public good' aspect of information. . . . The second . . . is related to the credibility of that information. It may be difficult or impossible for potential users to distinguish good information from bad. If so, the price of information will reflect its average quality. And this can lead to market failure if entry is easy for firms offering poor quality information.

This as may safely be assumed in unregulated settings. In the absence of intermediaries, unreliable information would have resulted in a suboptimal level of investment in Lebanon.

Financial intermediaries "will be the preferred form of information producer" (Campbell and Kracaw 1980) if entrepreneurs desire confidentiality (as modeled by Campbell 1979). A Pareto-preferred rational expectations equilibrium will emerge as long as it can be assumed that the intermediaries are producing reliable information. Without assuming "pathologically honest" information brokers, the moral hazard question may be resolved if there are capital barriers to entry into the information market or at least a context of law and order for enforcing contracts. Otherwise, however, a rational expectations equilibrium between investors and financial intermediaries will not ordinarily be achieved through market forces. Some sort of regime will be needed to make the market work. In Lebanon, as we shall see, the government had to impose order on the banking system in the late 1960s. Subsequently, when the government could no longer enforce contracts, much less the banking regulations, the bankers themselves had to keep up their own regime.

Iterated Prisoners' Dilemmas and Discount Rates

Regimes of this kind arise, or at least may be sustained, in response to dilemmas of common interest or common aversions (Stein 1983). The theoretically more interesting dilemmas, requiring collaboration rather than mere coordination, are those of common interests, as expressed in the prisoner's dilemma.

The two prisoners, it will be recalled, can either stick to their alibi when the district attorney separately interrogates them or confess to the crime. The sentence for sticking to one's alibi (cooperating) will be longer than that for confessing (defecting) if the other prisoner gives in to the DA's bait of confessing and going free. The payoffs may be summarized: $DC > CC > DD > CD$. The prisoners have a common interest in cooperating ($CC > DD$), but each is better off defecting, whether the other cooperates ($DC > CC$) or defects ($DD > CD$). Their rational expectation, given the DA's incentive scheme, is mutual defection. The actors' dominant strategies produce, in microeconomic language, a Pareto-deficient equilibrium.

Though the prisoners' rational expectation is mutual defection in a one-play game, mutual cooperation becomes possible if they can expect an indefinite future of interactions. In the iterated version of the game proposed by Axelrod (1984), the two players know their past "prison" of interactions, anticipate an indefinite future of interactions, and value present payoffs over uncertain future ones. They can escape the dilemma if they can rationally expect each other's continued cooperation. Axelrod proposes that they may reasonably cooperate rather than defect if, in addition to expecting an indefinite future of interactions, each player does not discount expected future values of the increments of collaboration as compared to defection enough to make the present benefits of the latter offset the former. The critical variable is each player's discount rate.

The Lebanese bankers operated, of course, with real discount rates, or opportunity costs of capital, and these may serve to flesh out Axelrod's model. The following exposition about Lebanese banking will lead into the model of a prisoner's dilemma among bankers who roll over doubtful loans. By such "thick" analysis of an international regime, throwing financial metaphors back into context (Sartori 1970), I do not intend to criticize Axelrod or Keohane, much less to deter others from letting their metaphors fly further afield—rather, a firmer grounding may lead to higher rebounds.

The Lebanese Banking Regime

In the 1950s and 1960s, Beirut was the principal trading entrepôt for the Middle East, including the oil states of the Persian Gulf. Its physical and political conveniences also made Beirut the region's principal information center for exchanging commercial as well as other sorts of intelligence, but the banks were primarily engaged in reducing transaction costs associated with domestic and regional trade. Letters of credit and short-term commercial loans, often triangular deals connecting Lebanese traders with their clients around the world, continue to be the banks' bread-and-butter business.

The commercial banks became involved, however, in financing industry with medium- and long-term credit. Formal capital markets never functioned properly (Hoss 1974, 38), for investors had little reason to believe that stock prices, manipulated by inside dealers, reflected publicly available information. Moreover, very little reliable information was available to the public: published financial statements could not reflect generally accepted accounting procedures because the Lebanese accounting profession was unregulated, its practitioners at the mercy of their wealthy clients. Most entrepreneurs, too, preferred to keep ownership within closed circles of family and friends rather than to sell equity on an open market.

The situation of the typical Lebanese entrepreneur was analogous to that of the manager in more complex financial markets who prefers inside debt to equity or bond issues so as to hide strategic plans or marketing secrets from competitors (Campbell 1979). The banks were attractive to the entrepreneur because banks "do not always know the final purpose for which loans have been granted. This is mainly due to the multipurpose activities undertaken by a large number of major bank clients" (Saliba 1981, 152). Not always knowingly (though better informed than other potential investors), the commercial banks, by rolling over short-term commercial loans—in effect translating them into medium- or long-term financial instruments—sustained optimal levels of investment in Lebanese industry.

The Origins of the Banking Regime

The Lebanese government undertook its first serious efforts to regulate the banking industry only after it was rocked by a major financial crisis. In October 1966 the Palestinian-owned Intra Bank, by far the largest of Lebanon's banks, ceased to meet its payments after a run on its deposits. Whether, as its defenders argued, the bank merely faced a temporary liquidity problem or, as its critics insisted, it had been mismanaged to the point of insolvency, the immediate cause of the crisis was that Intra had engaged in too much long-term lending of short-term deposits. In reality this bank was only the tallest tree in a forest of overextended and mismanaged banks (Moore 1983, 33–34). The crisis enabled the government to implement the Code of Money and Credit enacted in 1963 over the opposition of Lebanon's freewheeling bankers, represented by their Association des Banques du Liban (Asseily 1967, 16–22).

The new regime sponsored by the government consisted of a Supreme Banking Council headed by the governor of the Bank of Lebanon (as the new central bank was named) and performing "the function of a banking court with authority to impose controls and various degrees of penalties on banks, including the withdrawal of the banking license." An independent Banking Control Commission was set up at the central bank to supervise the commercial banks and recommend to the Supreme Council any action to be "taken over banks facing special difficulties or banks violating the rules and regulations prescribed by the Code of Money and Credit and the directives issued by the Bank of Lebanon or the Banking Control Commission itself" (Saliba 1981, 149). In the spirit of Lebanon's constitution, it was informally understood that the governor of the central bank would be a Maronite, the chairman of the Banking Control Commission a Sunni Muslim, and so on down the line, replicating the country's system of countervailing minorities.

Authorized to clean up the system, the Banking Control Commission closed down ten of Lebanon's remaining 85 banks, permitted four others to go into voluntary liquidation, and supported a moratorium against issuing new bank licenses (Hoss 1974, 32). The managers of some of the liquidated banks were brought to trial and convicted for lending substantial sums to friends and fellow board members without adequate prospects of repayment, and some twenty members or former members of parliament were implicated (Messara 1977, 151). The Banking Control Commission was not able, however, to impose ratios or other restrictions upon banks in need of "improving their special situations" (Hoss 1974, 26) before the government collapsed in 1975. It had not resolved the basic problem of mismatched maturities and tacit term-lending by rollovers. Banks were still permitted to make loans of up to 30% of their capital per individual to their shareholders, families, and friends (Banker Research Unit 1980, 116).

The Regime's Survival under Conditions of Anarchy

Despite the collapse of government, however, the banking system survived without a single bank failure between 1975 and 1984. Fluctuations in international interest rates were far more volatile and threatening to Lebanese liquidity than those upon which Intra had foundered (Ghattas 1971). When, as in 1979-80, Eurodollar rates skyrocketed, local banks were tempted to speculate in foreign exchange transactions at the expense of liquidity in pounds. Another factor that had contributed to the problem of Intra and other banks was the scarcity of short-term commercial lending opportunities in Lebanon relative to the supply of short-term deposits. The warfare naturally accentuated the disproportion, for the deposits kept coming in—remittances from abroad, salaries from militias, and high hashish sales—partially offsetting the general slowdown in economic activity, whereas the slowdown restricted short-term lending, tempting some banks into excessive long-term lending and real estate speculation. In response to these pressures the central bank tried on occasion to restrict credit and reduce the ceiling on the net foreign exchange positions banks were in theory allowed to hold.

The regime survived in no small measure because of the commitment, and indeed heroism, of the Lebanese central and commercial bankers and their employees, who dodged bullets, paid off rival militias, and managed to keep their doors open for all but a few months of a 10-year nightmare. But economic factors were also at work. The central bank's ample gold cover, safely stored (the writer suspects) in the vaults of the New York Federal Reserve, increased sufficiently in market value to keep the Lebanese pound more amply covered than any other freely convertible currency in the world. And, of greatest import, the underlying Lebanese economy was sufficiently resilient to support a free banking system. The value of Lebanese industrial exports (mainly to Saudi Arabia and Iraq) increased from $64 million in 1970 to $528 million in 1980 (Rivier 1982, 123). But this economic miracle begs the question of why bankers remained willing to roll over the loans needed to finance it—even after the Israeli invasion of 1982 destroyed much of Lebanon's industrial plant.

Bankers became entrapped in ever-growing mountains of debt, much of it in the form of nonperforming commercial loans. By the end of 1983, bad debts "rolled over on a yearly basis . . . were conservatively estimated at around 15 percent of total loans outstanding" (Iskander and Baroudi 1984, 207). The Lebanese bankers had become the prisoners of their clientele but had to keep pumping and priming them, as do the multinationals with their LDC debtors for lack of better alternatives. Writing off even that conservative 15% of their loan assets, amounting to 169% of their capital accounts (Banque du Liban 1984, 41), would have entailed collective bankruptcy. Instead they cooperated, resolving their dilemma of common interests by rolling over the loans rather than individually defecting. Like blind soccer players they continued to exchange unreliable information about their clients in a stadium where, as one leading politician observed, not even the law of the jungle prevailed, so dense and overgrown was its underbrush of class, regional, and international, as well as sectarian, conflicts.

The very failure of the Banking Control Commission to rationalize the system and foster specialized medium- and long-term credit institutions may have helped the banker-prisoners to adapt to the new wartime situation. Credit analysis remained, for the most part, as refractory to modern techniques as when Yusif Sayigh had interviewed Lebanese bankers (1962, 94). Despite professional credit analysis seminars introduced by a dynamic chairman of the Bank Control Commission in 1981 and 1982, with the help of leading U.S. banks and continued by the American University of Beirut's Business School in 1983, the Lebanese paid only lip service. As one leading banker explained, credit was a function of trust and friendship, not of projected cash flows based on tenuous, primarily political assumptions. The high number of Lebanese banks meant that only trust and friendship could prevent market failure and throw bankers who were not necessarily trusting friends into each other's arms. The logic of the system rested on a classic creditors' dilemma of common interests.

The regime survived anarchy because it did not need to rely on the authorities to enforce regulations and directives. The central bank exercised authority over weaker banks that had lesser stakes in the system—even so, without operative legal sanctions, its tactics smacked more of carrots than sticks. Authority over the larger banks seems to have been minimal. When, for instance, the Bank Control Commission once confronted a major bank head-on, insisting that it remedy its woeful capital inadequacy, the dynamic chairman of the Control Commission lost his job despite full support from the Governor of the central bank (himself the son of a Lebanese president and a potential presidential contender). The Christian who managed the bank in question, a former finance minister, successfully negotiated the support of the incumbent Sunni prime minister, who disposed of this bit of Sunni patronage and was anticipating future electoral expenditures—high in Lebanon by international standards, as Hudson noted (1968, 255). The successor to the Control Commission was equally unable to impose prudent liquidity and solvency ratios upon the banks.

Most borrowers eligible for government subsidies to cover damage incurred in the 1975–76 fighting had preferred privately to renegotiate their loans with the banks, thereby preserving mutual honor and confidentiality, rather than be listed as receiving handouts. To assess the prospects for continued renegotiations, it is useful to try to reconstruct plausible parameters for their prisoners' dilemmas.

The Prisoners at Work: The Model

Suppose, not unreasonably in the conditions prevailing in Lebanon between 1976 and 1982, that an entrepreneur enjoys a close, possibly family relationship with the owner-manager of a bank that enjoys a good reputation with other banks. The bank may exchange shares of the entrepreneur's loans with other reputable banks for shares of their loans to other equally well-placed entrepreneurs —normal portfolio diversification to spread the risks—or the entrepreneur may patronize several banks. Wishing each year to roll over the loan, the entrepreneur who can credibly threaten his banks with default yet promise ultimate repayment "when the security conditions in the country improve" then enjoys a position of strength analogous to the district attorney's in a prisoner's dilemma. In practice, more than two banks will be renegotiating their loan with the entrepreneur, but the flavor of the situation can be conveyed by imagining just two banks, locked into a prisoner's dilemma by the shared prospect of having to refinance the entire loan or lose its share if the other defects. Assume in this simplest symmetrical case that each bank's share, k, is equal and that the perceptions of the probability of ultimate repayment, p, communicated by the original lending bank, are believed by the other banks. Then the expected identical payoffs are as follows:

$DC = k(\text{loan})$
$CC = pk(\text{loan} + \text{interest})$, where p is the probability that the loan and interest will be repaid in a year.
$DD = 0$
$CD = p(\text{loan} + \text{interest}) - (1 - k)(1 + r)(\text{loan})$, where r is the discount rate, or opportunity cost, of the additional funds required to finance the debtor if the other bank refuses to roll over the loan.

Assuming perfect symmetry, the banks will have the same opportunity cost of capital. The problem, following Axelrod (1984, 207-08), whose w can be transformed into a real discount rate $r = (1 - w)/w$, is to determine the maximum value of r, or the minimum of p, the probability of repayment, for which the payoff, DC, for unilateral defection will be offset by the stream of future benefits from mutual cooperation. If the players employ Axelrod's tit-for-tat strategy, the defector can expect a total stream of payments $DC + DD/(1 + r) + DD/(1 + r)^2 + \ldots$, whereas the bank that rolls over the loan can expect a total stream of CC/r. Since $DD = 0$, cooperation on a tit-for-tat basis is defensible as long as

$$DC < CC/r$$

$$k(\text{loan}) < pk(\text{loan})(1 + i)/r$$

$$r < p(1 + i), \text{ or } p > r/(1 + i)$$

In other words, if $r = .25$ and $i = .15$ (plausible in 1981), future expectations of cooperation could offset the temptation unilaterally to defect as long as the chances of repayment were at least one out of five. Banks where opportunity costs of capital were a bit higher—up to 45% return on equity—could still reasonably roll over loans if they perceived the chances of repayment to be better than two out of five. As long as the bankers could believe there were some prospects of an eventual recovery of the Lebanese economy, there seemed little reason for not continuing to lend to one's kin or friends or, by sharing in other banks' loans, to friends of other intermediaries.

Strategies to Avoid Playing Chicken

The model helps to explain banks' strategies as well as why they might keep cooperating in rolling over commercial and industrial loans under conditions of general anarchy. The assumption of perfect symmetry can be relaxed to reflect some of the variances in the luxuriance of strategies, types of ownership, styles of management, and clienteles displayed by the 80-odd commercial banks operating in Lebanon. After 1977, when the moratorium on issuing new bank licenses was lifted, 10 new banks squeezed into the crowded market place in search of prime clients while a number of established banks were sold to aggressive new owners, enabling potential prisoners to escape.

The banks can be situated in strategic niches defined by their opportunity costs of capital and perceptions of credit risks. Banks with high opportunity costs of capital and "conservative" habits of estimating risk would prefer to reduce their portfolio of doubtful debts and escape playing iterated prisoner's dilemma games. But it was better to qualify as a prisoner than be perceived as a less credible chicken ($DC > CC > CD > DD$) (Oye 1985, 8). Chickens had less bargaining power than real prisoners because their threat of defection was less credible. If $CD > DD = 0$, then

$$CD = pL(1 + i) - (1 - k)(1 + r)\text{loan} > 0$$

$$p > (1 - k)(1 + r)/(1 + i)$$

A bank could be perceived as playing chicken if p (the banker's estimate of the probability that the loan would even-

Table 1. Profiles of Owner Nationalities and Loan Portfolio Growth Rates of the Top 15 Creditor Banks by Rank: 1974-1982

Rank				Annual Growth of Loan Portfolio
1974	1982	Bank	Ownership	1974-82 (%)
1	15	G. Trad-Crédit Lyonnais	French, Lebanese	11
2	9	Société Nlle. de la Banque de Syrie et du Liban	French, Lebanese	17
3	1	Banque Libano-Française	French, Lebanese	26
4		Citibank	U.S.A.	-8
5		Banque Nationale de Paris	French	9
6		Chase Manhattan	U.S.A.	-9
7		Moscow Narodny[a]	U.S.S.R.	4
8		Société Générale Libano-Européenne	French, Lebanese	14
9		Arab African International Bank	Egyptian	-12
10		Bank Almashrek	U.S.A., Lebanese	14
11	3	Fransabank[b]	Lebanese	22
12	4	Banque Audi	Lebanese	29
13		Banco di Roma	Italian	3
14	8	Banque Libanaise pour le Commerce	Lebanese	25
15		Bank of America	U.S.A.	-11
	2	Banque du Liban et d'outre-Mer	Lebanese	36
	5	Banque Byblos	Lebanese	31
	6	Banque de la Méditerranée	Lebanese	31
	7	Beirut Riad Bank	Lebanese	29
	10	Banque Sarader	Lebanese	44
	11	Bank of Beirut and the Arab Countries	Lebanese	35
	12	Crédit Libanais	Lebanese	27
	13	Banque de l'Industrie et du Travail	Lebanese	35
	14	Crédit Populaire	Lebanese	40

Sources: Association des banques du Liban 1974; Baz 1984.

[a] Growth of loans is calculated until 1981.

[b] The Banque Française Pour le moyen Orient was merged with Banque Sabbagh and renamed Fransabank.

tually be repaid) were sufficiently high, k sufficiently high, and/or r sufficiently low. The profile of the chicken player was that of an unprofitable family bank saddled with high shares of loans on which its outlook remained bullish.

Foreign banks, especially the big U.S. multinationals, were among the first to cut their loan portfolios, while Lebanese-owned banks took over many of the former's prime Lebanese customers' loans (and corresponding deposits). Although most of the big foreign banks retained a presence in Lebanon after the first rounds of fighting in 1975-76, their position relative to Lebanese banks altered dramatically. Table 1 lists by rank the top 15 creditor banks in 1974 and in 1982, along with profiles of their principal owners' nationalities and the average annual rates of increase in the size of their loan portfolios between 1974 and 1982. Lending remained as concentrated in 1982 as in 1974—the top 15 accounting in both years for slightly over half of the banking system's credit to the private sector—but foreign banks and Lebanese banks owned in large part by foreign interests removed themselves from the competition. The fully Lebanese-owned banks, seeking market share and having greater confidence, perhaps, in their borrowers, were more willing to become prisoners.

The larger aggressive Lebanese banks

could divide up shares of prime customers' loans no longer wanted by Citibank or its peers, whereas family banks that elected to remain small risked becoming ever more burdened by undiversified chunks of debt of friends or relatives. If a bank's share of a doubtful debt were high, however, it would not qualify as a player in prisoner's dilemma because it would be better-off rolling over the loan even if other banks defected. Always concerned to prevent any bank failures that would adversely reflect upon the system, the central bank kept ailing family banks in business and encouraged their sale, wherever possible, to outside investors or their merger with healthier banks. In one case it even permitted the new investors (headed by a former finance minister) to acquire the bank *without* its portfolio of doubtful debts.

Most of the aggressive banks, in turn, seem to have regained relatively healthy returns on their capital after a period, between 1975 and 1978, of plowing their revenues back into reserves ("provisions") for doubtful debts. Among the 10 of the 15 top credit banks that had published income statements since 1973, only 3 were not declaring consistent profits—averaging 27% return on equity—during those golden years of banking under conditions of anarchy, 1980 and 1981. Their go-go growth strategies, however, generally sacrificed profits for market share and resulted in steady decreases in capital adequacy (i.e., higher leverage and risk).[1] Staying small and nursing a less diversified portfolio, on the other hand, could be riskier because other banks would have less incentive to cooperate in rolling over any problematic loans. The high-growth, undercapitalized banks could at least be each other's prisoners rather than succumb to the largesse of the central bank.

To qualify as a prisoner rather than a chicken, a bank needed size and corresponding market power (and possibly political power) as well as profitability to keep its individual shares of dubious loans to a minimum. No prisoner's dilemma model fully captures the process of loan renegotiations between unequal partners (Telhami, n.d.), but occasionally, as with Sinno and Jabbour in 1982, the negotiations became public knowledge. The banks broke out of their prisoners' dilemmas and delegated the head of a leading Lebanese bank, a former finance minister, to renegotiate the loans. The Sinno and Jabbour Group in the end agreed to sell off some real estate (including interests in a nightclub in Rio de Janeiro) and convert its short-term debt into a medium-term loan. The exposure of the bank that renegotiated the package was reduced by more than two-thirds, to a mere 5% of the term loan; another prominent Lebanese bank, enjoying especially close relationships with both the lead bank and their common French banking connections, also had its exposure whittled down at the expense of other Lebanese and Canadian banks (Abu Zaki 1982; Restructuring 1982).

The Apparent Breakdown of the Regime

Banks could reasonably continue to roll over dubious loans until 1984, when the political and economic consequences of the Israeli invasion and its aftermath, including the bombing of the U.S. and French peacekeeping forces, became fully apparent. Much of Lebanon's industrial plant had been destroyed, and exports to the Gulf were adversely affected also by the uncertainties of overland transportation through Syria and reduced demand in Iraq and Saudi Arabia. Because taxes could not be collected, the government experienced alarming deficits and a burgeoning public debt. For lack of exports or foreign investment, there was a record balance of payments deficit in 1984. The central bank insisted, however, that "exchange controls cannot be a solution to the balance-of-payments problem because it would finally sweep away confidence in the Lebanese economy" (Banque du Liban 1985, 20).

The new phase in Lebanese banking was anticipated by a swindle. The manager of First Phoenician (purchased from First Chicago in 1982) made fraudulent loans (Summary 1984, 31–34) financed in part by interbank deposits at high interest rates and departed for Brazil in March 1984 (Iskander 1984). The central bank rescued the depositors and put the bank under new management, but might not other banks finance borrowers speculating in foreign exchange with similar results? Assuming that most banks still acted "legally" and restrained their financing of foreign exchange speculation, what were their new parameters for rolling over old loans as expectations hardened that the bottom had fallen out of the foreign exchange market for the Lebanese pound?

An expected depreciation rate, d, of the Lebanese pound must be included in the model. The temptation of cashing in a loan might then be offset by the present value of the rewards of future repayment if

$$r < p(1 + i)(1 - d), \text{ or}$$

$$p > r/(1 + i)(1 - d).$$

By late 1984 there seemed little reason not to expect a continuing annual slide of the pound against the dollar (or a basket of foreign currencies) of 50% or more. Consequently, the probability of repaying a loan had to be at least $1/(1 + i)$ for banks to fit the prisoners' model of cooperation and at least to break even ($r = d$). Yet interest rates were being kept well below the rate of expected depreciation; and peace seemed ever less likely, diminishing the probability of repayment of doubtful debts.

Why banks continued to roll over loans is not altogether evident. For customers who could never repay or who had perhaps run off to Brazil with no intention of repaying, Lebanese banks still had no alternative but to keep the loans on their books. There was no legal way of writing them off unless the customer were declared bankrupt, but without functioning law courts, bankruptcy could not be declared (Saliba 1981, 154). Solvent clients, however, had their cash "outside" Lebanon. Any bank's advantage in collecting loans in depreciating Lebanese currency could still be offset by the rewards of other ongoing business relationships with the client. Because banks representing 60% of the banking system's total assets (Iskander 1984, 3) had established branches in Paris, Geneva, London, and Brussels, they might still be rolling over some of the nonperforming loans under the shadow of a shared future.

Dilemmas of common interest had meanwhile supported a bankers' regime inside Lebanon during a period of anarchy lasting from 1975 until 1984 or so, independent of continuing relationships abroad. The banking system was also one of Lebanon's few institutions to have preserved the values of mutual intraelite accommodation enshrined in its prewar consociational "democracy" (defined by Kerr not as "the rule of the *demos* but simply the distribution of guarantees to the recognized factions coexisting in the country of the means to defend their minimum interests" [1986, 188]). The banks prevented market failure despite asymmetric information segmented along sectarian as well as along family lines. They often cooperated across sectarian lines—a big syndicated loan for a cement works at Sibline, near the Shouf mountain strongholds of the Druze, being a good political example. Lebanon's leading Christian- and Sunni-owned banks participated in Druze chieftain Walid Jumblatt's project, whereas the major Druze-owned bank did not.

The banks were almost as much political as financial intermediaries. They help explain not only financial cooperation during Lebanon's war but also a political anomaly, namely why the antediluvian political chiefs—despite a revolutionary situation in 1975 (Johnson 1983)—could return to power or at least fight over its absence. "Writing large" the banking interests back to their political nexus, Lebanon's multibank system effectively performed a function of elite integration, whether for consociational democracy or for the more authoritarian regime attempted by the Gemayels after the Israeli invasion.

"Writing Large": Capital Adequacy and Political Change

Unlike Austria or Holland, Lebanon never evolved mass parties of social integration, even among lower-middle class Maronites (Entelis 1974, 101–23), that would make the consociational formula work under modern conditions. Instead, clientage networks not dissimilar to the ones described by Albert Hourani (1968, 41–68) in eighteenth century "Syria" persisted in the Lebanon of the 1970s. In part, the civil war signified the breakdown of the patrons' control over socially mobilized and politically awakened masses. One astonishing feature of Lebanon's superstructure by the late 1970s, however, was the resurrection of many of the political patrons whose grips over their respective communities had been weakened in the heat of the battles of 1975–76. The status of these putative warlords was apparently hereditary—as attested by the careers of Walid Jumblatt, Bechir and Amin Gemayel, and many others (Khalaf 1980, 254–58; Messara 1977, 183–201).

Durable patron-client networks may be presumed to have permitted these families to retain their grip, even as their locus of economic influence shifted after the Second World War from rural landholdings to urban commerce (Dekmejian 1975; Harik 1975). Banking then became so central to Lebanese politics that, in alliance with other commercial interests, it prevented the building of an interventionist Lebanese state (Dubar and Nasr 1976, 67–75; Owen 1976, 28–31). The country was to be a safe haven for senior French and then U.S. multinational partners in the 1950s and 1960s. Clientelist practices usefully insulated the commercial center against sectarian pressures (Lemarchand 1981, 23) even as the specter of "confessionalism" paralyzed efforts to change the political system. But the commercial machinery that materially sustained the political networks also contributed to the elite's cohesion. Without denying its possible basis in a shared class interest, this cohesion was most visibly displayed in concrete interconfessional commercial ties centered in the banking system.

The banks contributed to intersectarian cooperation by sharing in the financing of entrepreneurs, thereby directly or indirectly supporting the clienteles of the political chiefs. By exchanging information and sustaining capital markets, they provided those tangible "first-order" resources needed to preserve durable patron-client structures (Moore 1977; Scott 1972). The greater the number of "competing" banks, in fact, the better the chances of keeping the elite mutually accommodating despite acerbic cleavages that confessional violence further embittered after 1975. The system, however, also eliminated some elites who appeared too powerful (anticipating the Palestinian Liberation Organization), notably those Palestinians who, through Intra Bank, had controlled a host of strategic industries such as the Middle East Airlines, the Port of Beirut, and the Casino du Liban. Portfolio diversification mitigated other cleavages among Lebanese in the sectors that counted most: the commercial and industrial sources of political revenues inside the country. Indeed the leading political families shared just as many dilemmas of common interest as the bankers, but the bankers were the necessary mediators. Dilemmas of cooperation are less amenable to rational solutions among actors who, unlike bankers, are too envious and "too clever" (Axelrod 1984, 23).

Obviously bankers without guns could not control warlords (though a couple of prominent bankers were considered to be the principal financial promoters behind the elections of two presidents in 1982). With or without guns, the political chiefs hardly seemed capable even of primitive sociability, much less of reshaping a consociational formula, which requires an accommodating political elite. Salman (1984) compared their discussions at Geneva and Lausanne in 1983 and 1984 to those of hashish addicts living out Egyptian illusions and realities after 1967 on a houseboat in Naguib Mahfouz's *Chatter on the Nile.* Unfortunately, political Lebanon cannot survive unless they can agree on a new form of consociation (Messara 1983), yet most moderate non-Christians view the open recognition and legitimation of confessional cleavages required to make a consociation work (Lijphart 1977, 103) as a step toward the partition favored only by some Maronite and Israeli interests.

The alternatives are bleaker: partition or the unwilling assimilation of remaining Christians in a Muslim majority. A dominating Khomeini-style Shia plurality would be as distasteful to other Muslims and Druze, not to mention most educated Shiites, as to the Christian minorities. As Ajami (1985) explains, "the fight in Lebanon now is not about the establishment of a state of the zealous; it is about the apportioning of power among the country's principal sects. . . . A Shia bid for power that tries to outrun the sectarian compact can succeed no better than the Maronite dream of a state cut off from its Arab environment" (pp. 792–93). The other alternative, partition, may be the outcome of another decade of turmoil and terror unmixing the confessions, but only the Maronite and Druze quarter of the population have safe mountain havens on this gory reading of the future. Partition cannot be the outcome of a deliberate political settlement because the conditions rendering its implementation possible would have already permitted a new consociation.

Toward a Redistribution of Financial Power?

It may still not be too late for the warlords to patch together some settlement. Were the political miracle to occur, the banking system would indirectly contribute to the new equilibrium. The "hell of the old politics resurgent" might yet be reconciled with a "benign utopia of finance" (Ajami 1981, 2) if hard financial logic prevails and the warlords stop playing their perverse games.

Most of the leading Lebanese creditor banks could not sustain their prodigious growth through retained earnings. An examination of their balance sheets and income statements shows that by 1982 capital adequacy (measured by [capital + reserves]/total assets) was woefully inadequate by international standards (Chaib 1983, 13, 17, 18). Most of the banks keeping adequate provisions for their loans, in addition to their capital and other reserves, had required substantial injections of new capital. The new sources of capital were predominantly Muslim and might therefore in time tend to equilibrate the banking system in ways that would support incremental political change within a consociational context.

Most Lebanese freely admit that the old consociational framework's allocations no longer reflect demographic or political realities. It is only possible to suggest the discrepancies between the official alloca-

Table 2. Constitutional, Demographic, and Financial Indicators of Power by Religious Sect

Religions Sect	Seats in Parliament	Percent of Total Population[a] (est. 1985)	Percent of Banking Capital in Top 15 Banks[b] as to lending 1974	lending 1982	net worth 1982
Maronite	30	21			
Greek Orthodox	11	6			
Other Christian	13	8			
Total Christian	54	35	62	47	32
Sunni	20	25			
Shiite	19	36			
Druze	6	4			
Total non-Christian	45	65	18	36	33
Confessionally indeterminate			20	17	35
Totals	99	100	100	100	100

Sources: Chamie 1981, 25, 85; Association des Banques du Liban 1974; Baz 1984, 215, 220.

[a] Estimated growth rates for each sect were projected from the 1932 census. Palestinian immigrants, some of whom became Lebanese citizens, are not included in these estimates of the Lebanese population.

[b] The allocations of capital among the top 15 Lebanese creditor banks of 1974 are calculated from capital book values, whereas the capital of the 1982 creditors includes other reserves and loan provisions as well. Only capital and reserves, not provisions, are counted for the 15 banks having the greatest net worth.

tions, indicated by parliamentary seats, and the demographic realities, estimated impressionistically (the last Lebanese census was undertaken in 1932, and subsequent administrative projections make political but no demographic sense)—without statistically valid margins of error—in light of recent research into the fertility of the respective communities.[2] The results are summarized in Table 2. Comparisons between the first and second columns suggest the extent to which the Maronites, Greek Orthodox, and other Christian communities are overrepresented in the Lebanese parliament at the expense of the Sunni community and especially at the expense of the Shiite community.

The top fifteen Lebanese-owned creditor banks can also be scrutinized, still more tentatively, with respect to their sectarian ownership in 1974 and in 1982. The third column of Table 2 indicates that Christians controlled 62% of the total capital of these 15 banks in 1974, whereas the Muslims and Druze had only 18%, the rest belonging to banks that seemed confessionally indeterminate.[3] With only three exceptions, the same banks occupied the top 15 positions in 1982 (fourth column), but their ownership had changed. New capital injections tended to be Sunni Muslim, mainly from the Arabian Gulf, but many of the large lenders were still inadequately capitalized. A fifth column in Table 2 represents a different, only partially overlapping, set of leading banks, the 15 in 1982 that were the most highly capitalized. In this set the share of Christian equity is further diminished, yet these are the banks that may be waiting in the wings to rescue overextended ones. In the event of peace, a competitive shake-out of Lebanon's overcrowded banking system is to be expected. Comparisons between columns four and five suggest that the predominantly Christian banks would be the most vulnerable to takeovers. The resulting capital structure might then support a predominantly Muslim allocation of political power, just as the pre-1974 regime supported a Christian one.

Such a shift in financial power would be discreet and subtle: bank owners and managers have never supported the political clienteles of leading politicians along exclusively confessional lines. The logic of loan portfolio diversification encouraged intraconfessional collusion among elites. The most prominent banks were impeccably rooted in other communities as well as their own. One senior banker scolded the present writer for writing of political services Lebanese banks might be performing (Moore 1983)—until a very prominent young politician breezed unannounced into his office, elegantly upholding the author by his sporting presence, to our mutual merriment. Newer banks sometimes lacked subtlety by factoring confessional segmentation into their marketing strategy, putting a token Druze on the board of directors, for instance, to penetrate markets in the Shouf, where the Druze were concentrated. Shared confessional roots might indeed give entrepreneurs better chances to borrow, expand, and then diversify their credit base, especially since so much violence has restricted people and information to ever-tighter communal circuits. Much of any renewed economic activity would in turn reenforce political leaders' clienteles.

Conclusion

Such visions of incremental change may seem utopian, but the most plausible alternative is an equally incremental, albeit violent, grinding down of geographical units into confessionally homogeneous subunits over another decade or two. If Lebanon became too bloody a laboratory to offer further illustrations of the theory of international regimes, the poor country would, having committed suicide, highlight certain aspects of international reality that seem recalcitrant to theory.

Iterated prisoners' dilemmas assume a pale world, hardly Mediterranean or Middle Eastern, in which economic interest has castrated the passions. A prisoner's dilemma cannot encompass actors who "seek to minimize the difference between their own returns and those of others" (Stein 1983, 134). Its payoff matrices cannot quite do justice to passions like envy (where perhaps $DD > CC$) or to sheer bloody-mindedness (where the other's cooperation may be a source of pathological anxiety: $DD > CD > DC > CC$) —nor even to the uncalculating spontaneity expressed in Rousseau's more famous parable of the stag hunt (1962, 171: $CC > DC > DD = CD$). Bankers may feel more comfortable with Axelrod's tit-for-tat strategies than most politicians or warlords, for status calculations in economic relationships are less likely to transform "a variable-sum game like the Prisoner's Dilemma into a strictly competitive struggle with no possibility for joint gains" than they are in security relationships (Lipson 1984, 15). At least the bankers understand discount rates.

In a real world of anarchy, one that may be all too close to international reality, Lebanese bankers indeed kept faltering capital markets from failing. Their dilemmas of common interests also helped to keep the country together and service the business clienteles of its warring elites. Their banking regime supports Keohane's contention that such regimes may survive the hegemony of their creator, in this case a consociational democracy of sorts, at least for a time. The banking system's adaptation to anarchy also supports Krasner's hypothesis (1983, 361) that international regimes, while depending upon the allocation of power among the state actors that promoted them, may feed back changes in these relationships.

But meanwhile, Lebanon is committing suicide. Refounding a consociation on which to rebuild its shattered economy and society may require a genie as well as more Syrian military commitment. Most bankers, while prepared to shore up consociational democracy, would favor any Napoleon to protect themselves from the political jungle. Their regime, like the politicians they befriended, is better viewed as the residue of a disintegrated political order than as the proxy for a Hobbesian sovereign. Collusion among financial intermediaries cannot reshape political reality; it can only recall it briefly and perhaps explain it, "grey in grey," after polity and economy have both broken down.

Lebanon may be a plague that should not infect the study of international forms of disorder. But if its internal turmoil is viewed instead as a microcosm of our maladies, then international regimes based on the financial logic illustrated by the Lebanese bankers may be equally pale and unsatisfactory substitutes for more political forms of cooperation.

Notes

I offer this essay in memory of Malcolm Kerr, who supported my work at the American University of Beirut, and of Jim Coleman, who gave me refuge at UCLA. I wish to thank my colleagues in UCLA's Political Economy Colloquia, Jeff Frieden, Mike Wallerstein, and Ahmed Enany for their stimulation. This essay was originally presented at the 1985 convention of the American Political Science Association, New Orleans.

1. Among the 19 Lebanese banks for which income statements as well as balance sheets were available over the years 1980-82, capital adequacy was regressed on the log of the growth of total assets and the ratio in 1982 of the spreads between interest received and interest earned to total assets—a basic measure of profitability. The two independent variables accounted for a corrected r square of .57 of the variance in capital adequacy. The T statistic for growth was −2.1, for spreads, 2.8.

2. Chamie (1981, 85) estimated the net annual percentage growth rates of the respective religious communities as follows: Shiites, 3.8; Sunnis, 2.8; Catholics (of whom 80% are Maronite), 2.0; Druze, 1.8; and non-Catholic Christian, 1.7. Projecting these rates from the 1932 census, the Lebanese population (excluding all Palestinian immigrants, some of whom became Lebanese citizens) was just over 3 million in 1985, distributed as indicated in Table 2.

3. The ownership of each bank was judged with respect to the confessional affiliations of its directors and its suspected sources of capital to be either Christian, non-Christian, or confessionally indeterminate; some had changed between 1974 and 1982. The judgments are subjective; one or two notable Muslim members of the board did not make an otherwise Christian or Franco-Maronite bank confessionally indeterminate if the notables seemed to be tokens without much capital behind them. Each bank's capital was totally allocated to one of the three categories.

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Forthcoming in June

The following articles, controversies, and research notes have been scheduled to appear in the June 1987 issue:

Paul R. Abramson, "Measuring the Southern Contribution to the Democratic Coalition."

Vinod K. Aggarwal, Robert O. Keohane, and David B. Yoffie, "The Dynamics of Negotiated Protectionism."

Jonathan Bendor, Serge Taylor, and Roland van Gaalen, "Stacking the Deck: Bureaucratic Mission and the Search for Alternatives."

Darrel Dobbs, "Rationalism and Heroic Reverence in Homer's *Odyssey.*"

Bernard Grofman, Guillermo Owen, Nicholas Noviello, Amihai Glazer, "The Copeland Winner and Reasons for Stability and Centrality of Legislative Choice in the Spatial Context."

Robert W. Jackman, "Political Institutions and Voter Turnout in the Industrial Democracies."

M. Kent Jennings, "Residues of a Movement: The Aging of the American Protest Movement."

George Klosko, Edward N. Muller, and Karl-Dieter Opp, "The Rationality of Collective Action."

Michael MacKuen and Courtney Brown, "On Political Context and Attitude Change."

Douglas Madsen, "Political Self-Efficacy Tested."

Samuel Merrill, III and Jack Nagel, "The Effect of Approval Balloting on Strategic Voting Under Alternative Decision Rules."

Edward N. Muller and Mitchell A. Seligson, "Inequality and Insurgency."

Richard Niemi and Joel D. Barkan, "Age and Turnout in New Electorates and Peasant Societies."

Alwyn R. Rouyer, "Governmental Performance and the Decline of Fertility in India."

CONTROVERSY

MODELING WAR AND PEACE

The appropriate model of international conflict and war has been much discussed in recent years, and considerable progress has been made in modeling. Nevertheless the assumptions, conditions, and specifications for international conflict models remain eminently debatable. In the December 1986 issue of this Review, Bruce Bueno de Mesquita and David Lalman presented a generalized expected utility model of international conflict. Here Seif Hussein takes issue with their model in various particulars, aiming to perfect and strengthen the model. Bueno de Mesquita and Lalman respond.

In their recent paper, "Reason and War," Bruce Bueno de Mesquita and David Lalman (1986) present a generalized version of the former's well-known expected utility model of conflict and war. In the following paragraphs I offer modifications of some of their arguments, qualifications and extensions of others. I am only interested in the theoretical arguments, and those discussed here appear in the same order as they do in "Reason and War." Moreover, these arguments are presented in order of increasing importance, thus the more significant points come later.

Monotonicity and Maximization

The two authors start by assuming that decision makers are expected utility maximizers. Thus, when confronted with two options *a* and *b* such that the expected utility from *a* is greater than that from *b*, the decision maker rationally chooses *a*. However, the authors soon introduce another assumption describing the manner in which choice is made. This second assumption is that "the probability of a nation [in a conflict situation] choosing to escalate a dispute is monotonic with its expected utility."

I argue here that the first of these two assumptions is superfluous. The monotonic probability of escalation assumption is meant in the authors' analysis to reflect the *continuity* of probability assessment as a function of the decision makers' expected utility. Thus, with monotonicity every increment in expected utility increases the probability of escalation.[1] Expected utility maximization, on the other hand, does not allow for continuous changes in the probability of escalation. In fact, under the maximization assumption the probability of escalation can take no more than the following three values:

$$P^i(Esc_i) = 1 \text{ if } E^iU_{ij}(Esc) > E^iU_{ij}(\overline{Esc})$$
$$P^i(Esc_i) = .5 \text{ if } E^iU_{ij}(Esc) = E^iU_{ij}(\overline{Esc})$$
$$P^i(Esc_i) = 0 \text{ if } E^iU_{ij}(Esc) < E^iU_{ij}(\overline{Esc})$$

where

$P^i(Esc_i)$ = probability that decision maker i decides to escalate a dispute with j

$E^iU_{ij}(Esc)$ = i's expected utility from escalation

$E^iU_{ij}(\overline{Esc})$ = i's expected utility from not escalating

The following picture illustrates the differences in the behavior of the probability of escalation under the expected utility maximization assumption (Figure 1–a) and under the monotonic probability assumption (Figure 1–b).

Figure 1a. Probability Distribution Under Maximization

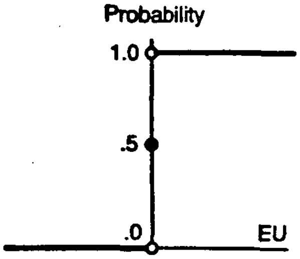
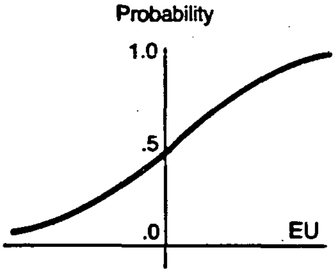


Figure 1b. Probability Distribution Under Monotonicity



Having established the differences in behavior implied by the two rationality assumptions, we note that the analysis in "Reason and War" can only be carried out under the monotonic probability assumption, making the maximization assumption altogether unnecessary.[2] Nonetheless, I shall make use of the latter assumption to illustrate a point.

The Necessary and Sufficient Conditions Reexamined

The authors make and prove the following most interesting proposition: "When the probabilities (of escalating a dispute) are monotonic with expected utilities the necessary and sufficient conditions for war/peace exist only in the limit." If no bounds are placed on the values of the utility of the acts, the necessary and sufficient conditions are satisfied when the numerical value of expected utility approaches *plus infinity* for war or *minus infinity* for peace.[3]

The above proposition is indeed true, the reason being that the necessary and sufficient conditions for war/peace obtain only if probabilities of escalation equal one/zero, respectively. Under the monotonicity assumption, where no bounds are placed on the values of expected utilities, these probabilities are reached only in the limit when decision makers gain their maximum expected utilities.

In the absence of the above proposition, one might be tempted to conclude that in an environment close to that of certainty, that is, when winning and losing are known with probabilities almost equal to one and zero, respectively, the necessary and sufficient conditions for war and peace will tend to be satisfied. Such a conclusion, however, will be wrong. This can easily be verified by noting that expected utility is computed by multiplying probabilities and utilities, and since the probabilities are bounded between zero and one, the only way for expected utility to approach the limit—assumed to be large enough and possibly infinite—is to allow the utility parts of the computation to increase or decrease freely. For example, if

$$EU = \Sigma^{k}{}_{t=1}\, p_t u_t$$

then lim $EU \rightarrow \infty\ [-\infty)$ only if

$u_p \rightarrow \infty\ (-\infty)$ for some u_p

and for all $u_s = u_p,\ u_s < M < \infty.$

k is finite.

The expression above holds true regardless of the values of p_t as long as these values are different from zero. Thus, if decision maker i is computing the expected utility from going to war with j solely on the basis of the probabilities and utilities of winning and losing, then the expected utility is evaluated as follows:

$$EU(war) = Prob(win) \cdot U(win) + Prob(lose) \cdot U(lose).$$

Now, even if the probability of winning is very tiny, as long as the utility from winning approaches infinity and that of losing is bounded by some finite number, the decision maker will escalate with a probability equal to one. If the same conditions apply for all relevant decision makers, then the necessary and sufficient conditions for war are satisfied. Similarly, if the utility of losing approaches minus infinity and that of winning remains bounded by a finite number, the necessary and sufficient conditions for peace obtain even if each decision maker is almost certain of winning the war (i.e., with probability close to one). In general, it can be shown that for any probability distribution $p = (p_1, \ldots, p_i, \ldots, p_m)$ where $0 < p_i \leqslant 1$, and any real number L, there exists an appropriate utility vector $u = (u_1, \ldots, u_i, \ldots, u_m)$ such that $p.u = L$. If the probabilities of winning and losing are viewed as a reflection of the power distribution in the system, then this result implies the generic nonexistence of power-based explanations of the necessary and sufficient conditions of war and peace.[4]

Now recall from Monotonicity and Maximization above that the one/zero probability is a feature of decision making under the expected utility maximization rule. Hence, in the limit, and only in the limit, do decision makers with monotonic probabilities of escalation act as if they are expected utility maximizers. Hence, the maximization assumption is also overly restrictive and of limited relevance to the analysis undertaken in "Reason and War." As is mentioned above, this assumption can be disposed of at no cost, and the entire analysis can be performed under the monotonic probability assumption.

The Opponent's Likely Behavior and the Decision to Escalate

Bueno de Mesquita and Lalman define $P^i(Esc_i)$ as nation i's probability assessment of escalating a dispute against nation j, and $P^i(Esc_j)$ as nation i's estimate of the probability that nation j will escalate. Then they give the following representation (under the assumption of monotonic probability):

$$P^i(Esc_i) = f(r \cdot cosine\theta) = f[E^i(U_{ij})] \quad (1)$$

$$P^i(Esc_j) = g(r \cdot sine\theta) = g[E^i(U_{ji})] \quad (2)$$

$$\text{where } r = \{[E^i(U_{ij})]^2 + [E^i(U_{ji})]^2\}^{1/2} \quad (3)$$

The multiplication sign is $\cdot$, and θ is the slope of r.

Notice that since both equations are estimates made by i, the shape of the function g must be known to i. If i then contemplates escalating a dispute against j, i must not fail to use this information to make the final assessment on whether or not to escalate. At least, that must be the reason why i should take the trouble and bear the cost of computing j's expected utility and guessing on the shape of j's reaction function, namely g. Thus, a final (probabilistic) assessment by i to escalate—we denote it as d^i(Esc)—should depend on i's welfare as well as its estimate of j's likely behavior, for example,

$$d^i(Esc) = d^i[E^iU_{ij}, g[E^iU_{ji})] \quad (4)$$

Hence, the basic four equations that the authors of "Reason and War" constructed and tested may be rewritten as follows:[5]

$$P(war) = d^i(Esc) \cdot d^j(Esc) \quad (5)$$

$$P(intervention) = \{d^i(Esc) \cdot [1 - d^j(Esc)]\} + \{d^j(Esc) \cdot [1 - d^i(Esc)]\} \quad (6)$$

$$P(peace) = [1 - d^i(Esc)] \cdot [1 - d^j(Esc)] \quad (7)$$

$$P(violence) = 1 - P(peace) = P(war) + P(intervention) \qquad (8)$$

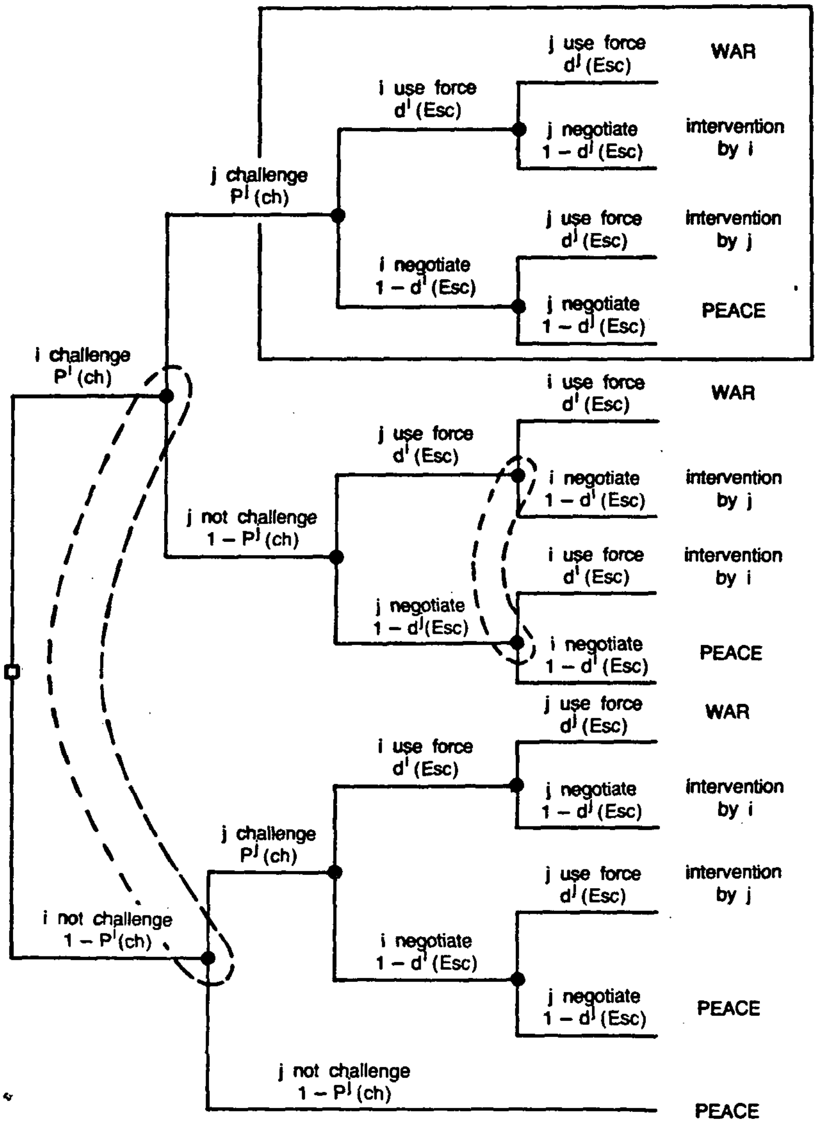
A rather peculiar feature of Bueno de Mesquita and Lalman's representation of Equations (1)-(3) above is that it does not allow for the kind of formulation we present in Equations (5)-(8). Indeed, if we try to rewrite i's probability of escalation—Equation (1)—as a function of i's estimate of the opponent's likely behavior—Equation (2)—using the information in Equation (3), we end up with Equation (1) again. So the formulation of the first three equations leaves no chance for i to benefit from already available vital information about its adversary's likely behavior, and changes along lines similar to the ones suggested are needed. I should also note that in the empirical analysis of "Reason and War," E^iU_{ji}, which is simply $g^{-1}[P^i(Esc_j)]$, is treated as an indicator of expected costs. This usage is certainly defensible, but it also invites the question, How can i have an estimate of expected costs and yet not use this information to revise and update the decision?

Escalation in Extensive Form

Now we turn to the last four equations, (5)-(8), and ask the following question: Are these equations acceptable after $P(Esc)$ is replaced with $d(Esc)$? The answer is mixed. On the one hand, our modified formulation does not go beyond a change of variables, leaving the original shape of the equations intact. Moreover, the original formulation, as well as the modified version seem to carry the right messages. On the other hand, these messages are incomplete. To illustrate, let us take Equation (7) and examine it along with Figure 3 in "Reason and War," which is the source of all four equations.[6] In Equation (7), probability of peace is estimated as the result of both nations' choosing not to escalate the dispute in question. But, as is clear from Figure 3, this assessment takes place only after i has decided to make a demand on j and a threatening one indeed. It is not unreasonable, however, to believe that peace may also prevail whenever i (and of course j) evaluates the expected utility from challenging the status quo and does not find it advantageous to do so. Hence, no demands are made. Therefore, the probability of peace should reflect not only the failure of a dispute to escalate into violence, which is what the authors of "Reason and War" offer, but also the probably more common situation in which both parties simultaneously decide that it is not in the interest of either of them to disturb the status quo.

To embody the above discussion in a probabilistic assessment of occurrence of peace as well as of war, intervention, and violence, we start by representing the problem of conflict escalation as a game in extensive form (Figure 2). This representation is most suitable for sequential decision making processes, when one of the players moves first and the other players move afterward. But in our case, we want to compute probabilities of outcomes generated by a sequence of events arising from either i or j challenging the status quo. This means that i and j must be allowed to move simultaneously. In a game in extensive form, such a situation is accounted for by allowing one of the players, say i, to move first and then not permit j to know the actual course of action i has taken. Hence the dotted information set around j's initial move in Figure 2, which reflects j's uncertainty about i's decision. However, after j makes a move, all information is disclosed and the game proceeds from there with the appropriate actor making the third move. Thus if i challenges the status quo and j does not, then after the information is disclosed, it is j who must react to i's attempt to upset the status quo. At this stage, j is allowed to use one of two strategies: the first is to use force to thwart i's challenge,

Figure 2. Escalation in Extensive Form.
The square in the upper right hand is equivalent to Figure 3 in "Reason and War."


j use force
d^j (Esc)
WAR
i use force
d^i (Esc)
j negotiate
1 – d^j (Esc)
intervention by i
j challenge
P^j (ch)
j use force
d^j (Esc)
intervention by j
i negotiate
1 – d^i (Esc)
j negotiate
1 – d^j (Esc)
PEACE
i challenge
P^i (ch)
i use force
d^i (Esc)
WAR
j use force
d^i (Esc)
i negotiate
1 – d^i (Esc)
intervention by j
j not challenge
1 – P^j (ch)
i use force
d^i (Esc)
intervention by i
j negotiate
1 – d^j(Esc)
i negotiate
1 – d^i (Esc)
PEACE
j use force
d^j (Esc)
WAR
i use force
d^i (Esc)
j negotiate
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intervention by i
j challenge
P^j (ch)
j use force
d^j (Esc)
intervention by j
i not challenge
1 – P^i(ch)
i negotiate
1 – d^i (Esc)
j negotiate
1 – d^j (Esc)
PEACE
j not challenge
1 – P^j (ch)
PEACE

and the second is to negotiate a way out. To this i responds either by using force to effect its desire or by negotiating an agreement with j. The probability of each action is depicted underneath that action in the decision tree. No payoffs are shown, only final outcomes: war, peace, or military intervention.

Two important points are to be emphasized. The first is that there is no reason to restrict the strategy set of each player to the binary choice of *either use force or negotiate*. Giving in to the opponent's demands is definitely an admissible strategy and so is a *blend* of *use of force* and *negotiate*. Outcomes corresponding to these alternative strategies can be different levels of violence and/or peace. I do not wish to complicate this brief note further by accounting for all admissible strategies. Nonetheless, a detailed study of war and peace must take them into consideration. The second point is that the final stage of the game can arguably take place in ignorance of the outcome of the stage that preceded it in that i and j may be making simultaneous decisions. To accommodate such possibilities, I depicted only one information set at the middle of Figure 2. The reader must decide for himself or herself if more are needed.

Now with the above discussion in mind and guided by Figure 2, Equations (5)–(8) can be rewritten, allowing the possibility of new interpretations for the probabilities of war, intervention, and peace. These are given below as equations (5′)–(8′). To simplify notations I let p^i, d^i, and $(1 - d^i)$ *denote the probability that i* will *challenge*, *use force*, and *negotiate*, respectively. Similar expressions for j are needed.

$$\begin{aligned} P(war) &= [P^i \cdot P^j \cdot d^i \cdot d^j \\ &+ (1 - P^j) \cdot P^i \cdot d^i \cdot d^j \\ &+ (1 - P^i) \cdot P^j \cdot d^i \cdot d^j] \\ &= d^i \cdot d^j \cdot (P^i + P^j - P^i \cdot P^j) \end{aligned} \tag{5′}$$

$$\begin{aligned} P(intervention) &= P(intervention\ by\ i) \\ &+ P(intervention\ by\ j) \\ &= [P^i \cdot P^j \cdot d^i \cdot (1 - d^j) \\ &+ P^i \cdot (1 - P^j) \cdot d^i \cdot (1 - d^j) \\ &+ (1 - P^i) \cdot P^j \cdot d^i \cdot (1 - d^j)] \\ &+ (P^i \cdot P^j \cdot d^j \cdot (1 - d^i) \\ &+ P^i \cdot (1 - P^j) \cdot d^j \cdot (1 - d^i) \\ &+ (1 - P^i) \cdot P^j \cdot d^j \cdot (1 - d^i)] \\ &= [d^i \cdot (1 - d^j) \\ &+ d^j \cdot (1 - d^i)] \cdot (P^i + P^j - P^i \cdot P^j) \\ &= (d^i + d^j - 2d^i \cdot d^j) \\ &\cdot (P^i + P^j - P^i \cdot P^j) \end{aligned} \tag{6′}$$

$$\begin{aligned} P(peace) &= [P^i \cdot P^j \cdot (1 - d^i) \cdot (1 - d^j) \\ &+ (1 - P^j) \cdot P^i \cdot (1 - d^i) \cdot (1 - d^j) \\ &+ (1 - P^i) \cdot P^j \cdot (1 - d^i) \cdot (1 - d^j) \\ &+ (1 - P^i) \cdot (1 - P^j)] \\ &= (1 - d^i) \cdot (1 - d^j) \\ &\cdot (P^i + P^j - P^i \cdot P^j) \\ &+ (1 - P^i) \cdot (1 - P^j) \end{aligned} \tag{7′}$$

$$\begin{aligned} P(violence) &= 1 - P(peace) \\ &= P(war) + P(Intervention) \end{aligned} \tag{8′}$$

The following final comments are in order:

a. The *challenge* and *no challenge* options are usually treated as primitive alternatives, that is, expected utilities are computed over them and a choice is made to follow one or the other. Generally, no probabilities are placed on these alternatives, but it is no difficult matter to construct such probabilities.[7]

b. Equations (5′)–(7′) above are clearly more general than the corresponding ones in "Reason and War." In fact, with the proper specification, the latter equations can all be derived from Equations (5′)–(7′) above.

c. Although Equations (8) and (8′) retain the same shape they had in the formulation in "Reason and War," the components of each carry information quite different from that in the original one.

d. It must also be emphasized that Equation (7′) computes the probability of peace between the pair *i* and *j* only. To find the probability of peace for the whole system, the computation of Equation (7′) has to be performed on all pairs in the system; then all these computations have to be compressed in one final probability assessment. Similar expressions developed along the same lines generate systemic probabilities of war, intervention, and violence.

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Seif Hussein suggests modifications and expansions of the expected utility approach to international conflict. These enhance the generality and the interpretation of our propositions. He shows that weaker assumptions consistent with our approach yield the same predictions. Hussein notes that his points "are presented in order of increasing importance, thus the more significant points come later." Our response, then, begins with his more important statements and proceeds to those he considers of lesser consequence.

His four major arguments, in descending importance, are

a. *Escalation in extensive form* can account for all conditions leading to peace. "Reason and War" accounts only for alternative resolutions of conflicts, given that a crisis has occurred (Bueno de Mesquita and Lalman 1986; Lalman 1985).

b. The extensive form that he suggests can be used to define systematic relationships that underlie the *opponent's response behavior.*

c. The propositions in "Reason and War" provide an important additional *critique of power-based theories* not drawn out in the article.

d. The *assumption of expected utility maximization* is superfluous for the main results in "Reason and War."

Escalation in Extensive Form

Seif Hussein offers several interesting extensions of the argument in "Reason and War." His Equations (5′)–(8′) are equivalent to Expressions (1)–(4) in "Reason and War" *if* one assumes, as we have in our paper, that leaders in nation *i* have already chosen to challenge *j*. Thus, Hussein's p^i (the probability of a challenge by i) is given as 1 in "Reason and War" since we examine only instances in which a conflict already exists. To show the equivalences, substitute the value 1 for p^i in Hussein's Equations (5′)–(8′). These reduce directly to our equations (1)–(4). Equations (5′)–(8′) are more general than the equations that describe the same conditions in "Reason and War," permitting analysis of the likelihood of no crisis arising in the first place. That is, they include those instances of peace that occur in nonconflict cases.

Hussein's contribution is a direct theoretical extension of our model and as such is readily testable. By our strict monotonicity assumption, as Equation (1) from "Reason and War" approaches its minimum value, *not challenging* is increasingly preferred to *challenging* the putative opponent. Therefore, as the overall expected utility gets smaller for any nation *i*, the probability that no crisis will arise (i.e., that there will be the peace that follows the continuation of the status quo) increases. To test this extension, we create a random set of 240 ordered pairs of nations (*i,j*) such that *i* is a prospective challenger and *j* is the prospective opponent of *i*. The selection criterion is that *i* and *j* must not have been engaged in a threat, intervention, or war with one another. Our expectation is that the mean $E^i\ (U_{ij})$ score for the randomly chosen i will be less than for those nations i engaged in any level of dispute (i.e., our data set of 133 conflict events used in "Reason and War"). A comparison of the mean expected utility scores for i in the two data sets reveals that the values in the nonconflict cases are smaller than those in the conflict cases. The t-statistic is 5.5, indicating that the difference in means is extremely unlikely to have occurred by chance. Thus, Hussein's extension is an application of the logic in "Reason and War" to the problem of conflict initiation, a subject not addressed in our paper. The result reported above highlights, in a preliminary way, the potential of our model to account for initiation as well as escalation of disputes. In fact, initiation is examined in *The War Trap* (Bueno de Mesquita 1981) and is the subject of work in progress by us. Hussein has anticipated our extension of the model to nonconflict cases.

Opponent's Response Behavior

Hussein's criticism that i inadequately takes into account the expected behavior of j is correct as far as it goes. His reasoning is appropriate to a sequential game, while our paper is about the initial decision by i or j to escalate. It is not about subsequent rounds of negotiation or escalation. In our paper we allow some updating of i's decision through the mechanism of controlling for i's belief about j's expected utility relative to i's. Thus we introduce dummy variables to identify in which octant the ordered pair of expected utilities $[E^i\ (U_{ij}),\ E^i\ (U_{ji})]$ fall and use that information to estimate the costs i expects j will inflict on i. We agree that j's position is important in the unfolding of a conflict but disagree over its significance for i's *initial* decision to escalate. Consider the payoff matrix below:

Table 1. *i*'s **Approximation of** *j*'s **Initial Choice**

i's Initial Choice	Use Force (*Prob* = *P*)	Negotiate (*Prob* = 1 − *P*)
Use force	$P(F_i - C_i)$	$(1 - P)F_i$
Negotiate	$P(N_i - C_i)$	$(1 - P)N_i$

where F_i is i's expected utility from choosing the *force* strategy, P is the a priori probability i attaches to j's selecting its force strategy, C_i is i's estimate of the cost to i if j chooses the *force* strategy—$g[E^i\ (U_{ji})]$ from "Reason and War"—and N_i is i's expected utility from choosing the *negotiate* strategy. A similar set of payoffs could be specified from j's perspective. Now if i chooses *force*, the expected value will be

$$PF_i - PC_i + F_i - PF_i = F_i - PC_i. \qquad (1)$$

If i chooses the *negotiate* strategy, the expected value will be

$$PN_i - PC_i + N_i - PN_i$$
$$= N_i - PC_i. \qquad (2)$$

I's choice of strategy is based on

$$(F_i - PC_i) - (N_i - PC_i)$$
$$= F_i - N_i. \qquad (3)$$

Therefore, taking i's perception of j's expected utility into account, we see that in the *initial* choice of strategies by i, only i's expected utility and not i's view of j's expected utility affect i's decision to use force or negotiate. This is as it should be since the expected utility estimates are constructed as net changes in welfare, given that the potential opponent is willing to use all of its capabilities to inflict costs on i (as indicated in the specification of the value of P_i^i in "Reason and War"). We treat i as updating its estimate of likely costs away from the worst case scenario through the use of $E^i\ (U_{ji})$ to determine in which octant the dispute lies, given $E^i(U_{ij})$. The logit coefficients for the cost variables in each octant are empirical approximations of the function g.

In later research we plan to move toward a sequential decision analysis to develop theoretically determined estimates of g. We share Hussein's view that this would be superior. The form of g and how i evaluates the information provided by g are not yet determined either by us or by Hussein.

Critique of Power-based Theories

Hussein provides a demonstration that Propositions (1) and (1a) of "Reason and War" indicate why power-based theories of international conflict are inadequate. This is a useful elaboration that reinforces arguments we have made elsewhere (Bueno de Mesquita 1981a; Lalman 1985). Indeed, in "Reason and War" we note that "the simple theoretical structure set out here and elaborated more fully in Lalman (1985), encompasses structural theories of power distributions, arms races, deterrence, negotiations, bluff, and escalation." In subsequent work, we plan to pursue more thoroughly the implications of the expected utility approach for power-based and other theories of conflict.

Maximization Assumption

Hussein has very cleverly demonstrated that the assumption of expected utility maximization is a proper subset of the assumption of monotonicity. Therefore, our assumption of strict monotonicity—accompanied by our implied assumption that the monotonic relationship between expected utility and the probability of escalation is differentiable—is sufficient to deduce the results in "Reason and War." However, when investigating conflict initiation and other dimensions of conflict decision making—all of which are part of our larger research program—the maximization assumption is still useful.

The monotonicity assumption is much less restrictive than the assumption of expected utility maximization, while still being consistent with rationality under the following conditions:

a. If i is rational in the expected utility sense, then when faced with discrete strategic choices, i selects the better (i.e., larger expected utility) option for sure. However, i does not know the precise shape of j's utility function and so can only guess probabilistically at what strategy j will choose (see Table 1). That probabilistic calculation is assumed to be monotonic and differentiable with i's estimate of j's expected utility (and with j's estimate of i's expected utility).

b. We, as researchers, can only estimate i's and j's expected utility values. Therefore, our predictions of their behavior are probabilistic (monotonic and differentiable) with our estimates of their expected utilities.

What is gained with the monotonicity assumption is the ability to explain variations in choices within the ordinal categories identified in Proposition 2 in "Reason and War." Hussein's comments highlight the fact that stronger theoretical and empirical results have been obtained while employing the weaker assumption that the choice of a strategy is a monotonic and differentiable function of the utility expected from that choice.

The thrust of Hussein's comment, our response, and the body of literature using expected utility theory to account for international conflict is that such an approach is moving toward an axiomatic understanding of the subject. This being the case, we can evaluate the impact of individual assumptions on subsequent deductions. In this way we can weaken assumptions, helping to make the results even more general. Hussein has pointed the way toward making improvements along this line without altering the fundamental aspects of our approach.

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Notes

1. To be more precise, the term *monotonicity* is used to mean that the probability of escalation is strictly increasing and twice continuously differentiable in its argument. See Lalman 1985.

2. From a technical point of view a weaker form of the monotonicity axiom can be satisfied by the maximization assumption. But that would be at the expense of the continuity of probability estimates. Continuity and monotonicity are indeed the driving forces of "Reason and War."

3. One can indeed place bounds on the upper and lower limits and the proposition will still hold true.

4. Similar results can be found in Bueno de Mesquita 1981 and Lalman 1985.

5. These formula also appear in Lalman 1985.

6. Figure 3 from "Reason and War" is depicted as part of Figure 2 below.

7. See Hussein 1986 for such a construction.

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RESEARCH NOTE

RECRUITMENT CONTRASTS IN A DIVIDED CHARISMATIC MOVEMENT

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This research report analyzes the evolution of the bond between a charismatic leader and his mass following. Its empirical focus is the case of Juan Perón and the Peronist movement in Argentina. After discussing stages in the evolution of charisma, we review our earlier findings showing that by 1965 there was clear separation between those Peronists devoted to the movement and those Peronists devoted to the man. We note distinctive features in the social and attitudinal profiles of each and then turn to our principal concern, the recruitment of new blood into each Peronist camp. Personalist youth turn out to be much like their older counterparts, but organizationalist youth demonstrate social and attitudinal features that set them dramatically apart from the personalists and apart from the older organizationalists as well. We discuss the implications of this ever widening rift.

Many fascinating and important phenomena in political life, though in principle available for empirical study, in practice present intractable problems to the would-be systematic investigator. As a result they are left in a condition of scientific limbo sometimes described as theory-rich but data-poor. Political charisma is one such phenomenon. The difficulties its study presents to the empiricist are formidable and doubly so if the charisma in question involves mass publics rather than small groups. How can one collect crucial attitudinal data in such an intensely emotional setting? How can one systematically observe the evolving bond between leader and mass? In this light it is not surprising that empirical work on political charisma is so very meager. This research note comes from a larger study that will provide data-based—though necessarily still preliminary—answers to some important questions about that phenomenon. The study has as its empirical focus Argentina's Peronists and their leader, Juan Domingo Perón.

In an earlier report, we laid out our theoretical perspectives, data sources, analytic methods, and three key findings: (1) two variants of Peronism had emerged after Juan Perón's initial period of power, one faithful to the man, the other more devoted to the movement; (2) these two wings of Peronism had different social and attitudinal bases; and (3) to a significant extent the social base of a wing predicted its attitudinal base (Madsen and Snow 1983).

In this note we take up recruitment. Where does each of the two variants of Peronism find its new blood? What factors drew new members into each of the two wings? Casting these questions into stark relief is the fact that by the time of Perón's return to power in 1973, the two wings of Peronism were in full-scale combat. Indeed, the airport fighting between them at the time of Perón's intendedly triumphant return to Argentina has been popularly labeled the Ezeiza Massacre (Page 1983, 579). What had happened?

Some Background

Juan Perón was a little known member of the military circle that seized the Argentine government in 1943. This coup came after a period of economic crisis marked by major decline in the all-important agricultural sector, by massive migration to crowded and job-short urban centers, and by poverty and despair among urban workers. Perón became secretary of labor in the new government and for two years, with wide publicity and great drama, used his powers to transform the lives of workers and their families. Not surprisingly, he acquired a passionate following. Threatened by Perón's ascendancy, the army arrested him in October 1945, an act that brought tens of thousands of his supporters marching in massive columns from the outlying slums and industrial suburbs to the center of Buenos Aires. They demanded and obtained his release.

For the next decade, as master of Argentina, Perón led the government. He also transformed the structural bases of Argentine politics, creating and nurturing Peronist party and trade union apparatuses and bringing many previously unrepresented elements (including women) into those organizations. In 1955, growing disputes with the Church (from which he was excommunicated) and with the armed forces, coupled with economic reversals, led to a coup that forced Perón into exile.

Even while in exile, Perón continued to be the major factor in Argentine politics. Leaders of Peronist organizations streamed back and forth across the Atlantic, seeking guidance and bringing back policies and taped speeches for the faithful. Although the Peronist party had been declared illegal, Peronist trade unions continued to function. Eventually, in the congressional elections of 1962 and 1965, Peronist candidates were allowed, but they could not be identified as such on the ballot. The results of both these elections showed Peronism still to be the nation's largest party, and both led to military coups. In 1973 the Peronists were allowed to run a candidate for the presidency, as long as that candidate was not Perón himself. A stand-in named Héctor Cámpora ran with the slogan "Cámpora to the government, Perón to power." He won, and three months later resigned, necessitating new elections. This time Perón was nominated, and he won with 63% of the vote. He returned to Buenos Aires on 23 June 1973 to the sound of gunfire. His followers were killing one another.

There can be no surprise in a discovery that in the 30 years Perón held center stage in Argentine political life, his bond with his followers had changed. After all, the Peronists, from an inchoate though passionate agglomeration had become a structured movement. Perón himself, in government and even more in exile, had to rely on lieutenants and agents—intermediaries who carried their version of his Word to the faithful and who put their version of his policies into effect. Argentina's economy, though having ups and downs, had left behind the dismal conditions under which Perón's charismatic leadership began. And, of course, the very personnel of Peronism had changed as old followers dropped out or died and new ones were recruited.

Edward Shils (1958) has argued that a charismatic bond inevitably evolves from a direct, concentrated, and emotionally intense form at its birth to an indirect, dispersed, and less passionate one as it ages. If this is true in general, what would the particulars be in the Peronist case? What sorts of followers would still have a direct and emotional tie to Perón as a man? What sorts would be more attached to Peronism as a movement?

In seeking to answer those questions, we were fortunate to obtain data from a national survey taken almost exactly midway between Perón's expulsion from Argentina and his triumphant return. (The methods of the survey are described in Kirkpatrick 1971.) In such a serendipitously timed—though, it turned out, still far from ideal—set of observations, we had a unique resource within which to examine the status of Peronism twenty years after its inception.

The survey data made it clear that the Argentine public was making a distinction between Perón and Peronism. Those items dealing explicitly with Perón himself exhibited one pattern and those dealing with the movement quite another. Factor analysis (with an orthogonal rotation) was ultimately used to construct measures of personalist and of organizationalist tendencies. And then 100 respondents were drawn from the uppermost location on each factor, yielding two pure groups for further analysis. Our pure personalists were unwaveringly committed to Perón but typically rather lukewarm to the movement. Our pure organizationalists were strongly committed to the movement but on average somewhat negative in their feelings for the man. But remember that in both cases these attachments were (by definition) affective and symbolic; they do not involve formal membership of any kind.

The socioeconomic status (SES) differences between personalists and organizationalists are striking. The former were far more likely to be residents of small towns or recent migrants to cities. Most interesting, they were, in SES terms, very much worse off than were the organizationalists.

Temperamentally, the personalists were more convinced of the value of strong leaders, more acquiescent to authority, and more confused about politics and government. This profile surely holds no surprise for students of authoritarian politics. And recall, this is a profile that correlates highly with the socioeconomic profile—that is, the latter substantially predicts the former in a statistical sense. These results leave the strong impression that in our personalists we have people much like those who in the 1940s found their savior in Juan Perón. Moreover, they seem most unready to give up their attachment to their leader. The forces behind the dispersion of Peronist charisma would seem most unlikely to flourish here.

The organizationalists were not only better off in SES terms but were also distinctive attitudinally; notable was their more conventionally "leftist" response to worker and communist symbols.

Recruitment Contrasts

The contribution of this note is to add consideration of recruitment processes to our analysis of the evolution of charisma. What sorts of youth were coming into each wing of Peronism? Was the social and psychological makeup of the recruits like that of their older counterparts, suggesting a stable gap between the two groups, or was that makeup different?

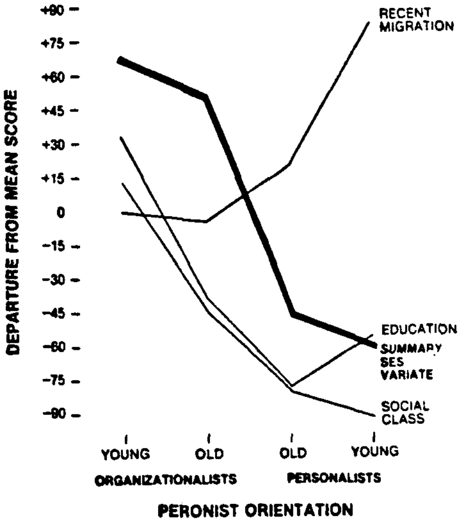
We assumed that the conditions leading large numbers of people to seek a personal savior in political life would be much the same, whether they were young or old—or, for that matter, whether they were 1965 seekers or 1945 seekers. We were more uncertain about the organizational young. No a priori reason led us to expect them to differ greatly from their older counterparts, one way or the other.

Before turning to the evidence, we must insert an important caveat. The data speaking to our recruitment questions are much less firm than we would have liked. Using an age division point of 30 years, we have one-fifth of our 100 cases in each youth component. Thus, we are here examining 20 cases of personalist youth and about the same number of organizationalist youth. While these figures do not seem unrealistic in terms of population parameters, they do illustrate the problem with using general population samples in analyses of subgroups. The evidence presented below must be considered in that light. Our own view is that the persuasiveness sacrificed by the data in purely statistical terms is substantially recovered by two other tests of data adequacy: strong empirical patterns and a clear fit with other sources of evidence (see di Tella 1983, chap. 2; Gillespie 1982; Guimaraes 1982; Page 1983, chap. 47; and Smith 1982).

Socioeconomic Differences

In our youth groups we have individuals who were no more than 10 years old and in some cases yet unborn when Perón's initial bond with his following was formed in 1945. They were between 8 and 20 when he went into exile. How does their socioeconomic makeup compare with that of their older counterparts?

Figure 1. Age Cohorts in Charismatic Dispersion: Socioeconomic Factors



The personalist young and old were, as expected, much the same. It is true that the young were a bit poorer and a bit better educated, but the only striking difference had to do with recent migration, which was much higher for the young than for the old (who were themselves well above the population average).

With respect to the organizationalists, in most of the social and economic conditions of young and old, we found a reasonably close match. But there was one astonishing exception: the young were of much higher social status, scoring even above the average for the Argentine population as a whole, while their older counterparts scored well below average! In class terms, then, these young organizationalists were very different indeed.

In Figure 1 we present the key findings. Given in bold relief is the line of position for each group on a summary socioeconomic measure (standardized across the 200 Peronist cases to produce a mean of zero and a standard deviation of one). This measure, called a *canonical variate* in technical language, was derived from an earlier discriminant function analysis in which weights were assigned to social and economic variables on the basis of their capacity to discriminate between personalist and organizationalist Peronists (full details are given in Madsen and Snow 1983). Note that in this weighting process, age was not involved.

On this summary measure, we can see in Figure 1 that the two youth groups are even farther apart than are their older counterparts—a finding that portends not stability or convergence but rather a growing gap between the two wings of Peronism. In their social foundations they apparently will have less and less in common.

The particulars of this growing gap are also to be found in Figure 1. The three factors most powerful in differentiating the newer recruits from the older members are recency of migration, social class, and education. (In the figure, each of the three has been standardized across the entire sample, which permits group scores to be compared with population averages, as well as with each other.)

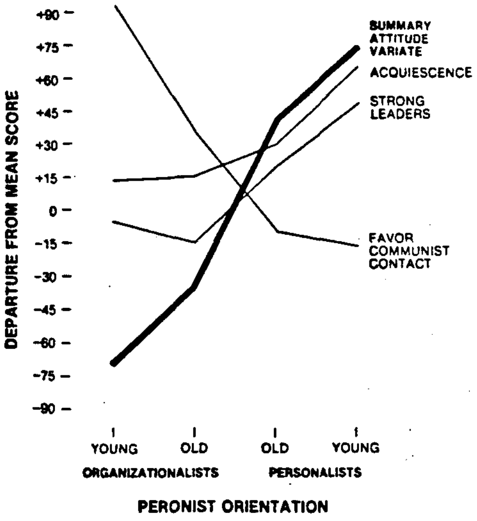
Psychological Differences

The temperamental differences between young and old personalists can be quickly summarized. The young felt slightly less able to understand politics (in spite of greater education); they were much less likely to promise support to political candidates from the working class; they were much more supportive of a strong leader in politics; and they scored much higher on a measure of acquiescence to authority. If personalists in general found happiness with a *leader*, these young personalists must have found rapture!

A comparison of young and old organizationalists on the attitudinal items revealed one remarkable difference: the young gave solid support to a policy of friendly Argentine relations with Cuba and with unspecified "communist countries." As noted earlier, the organizationalists had already been found to be more leftist than the personalists, but here we find the young going far beyond the position of the typical older organizationalist. Such a result is all the more striking when one realizes that this leftward drift could find no sanction in Perón's own rhetoric; indeed, quite the opposite.

In Figure 2 the attitudinal findings are given. Again presented in bold relief is the line of position for each group on a summary canonical variate, the one built from the attitudinal items that differentiate personalists from organizationalists. Plainly, the positions of the two youth groups here reinforce the impression given by the socioeconomic comparisons: if these two groups represented the future of Peronism, a deep schism in the movement was assured.

Figure 2. Age Cohorts in Charismatic Dispersion: Attitudinal Factors



Discussion

The evidence presented here suggests the possibility of a development in the evolution of Peronism that in fact occurred seven years later: Peronists warred with Peronists. The character of the warring camps is quite well predicted from the results given above. This remarkable fit with subsequent events adds a good deal of interest (and additional strength) to our findings.

One might ask, however, what the Peronist case teaches us about charismatic movements in general? If we can say that the nature of the original Peronism was certain to change, that inevitably there would come greater differentiation within the movement, can we also say why the change took the shape it did? Was simple disintegration equally likely? Are there general lessons about the role of new blood in the dispersion of charisma that can be drawn on the basis of these findings?

We believe our findings on the personalists to be general. The main point is simple: people in crisis are ready for—and sometimes actively seek—a personal savior; once they have found one, they do not easily let go. Of course, crises take many shapes, sometimes being more microscopic in their character and other times more macroscopic. Social scientists have a tendency to notice only the latter.

Personalists found in Juan Perón the answer to their own crises (which were triggered by macroscopic events and hence had macroscopic scope). Organizationalists, on the other hand, having escaped those circumstances—or, in the case of the young organizationalists, never having faced them—saw Perón himself in a more dispassionate light. For them, the charisma had been dispersed.

If this is correct, it suggests that all charismatic movements must founder in the face of success. When the crisis has passed, for whatever reason, the followers regain their sense of control, their sense of personal efficacy, and the leader recedes into the psychic shadows. However, any structural residue from the charismatic period may by that time have gained bureaucratic life of its own and survive.

Of course, none of this tells us why the young organizationalists were moving to the left. Obviously, there is no answer to be found in our data. One might speculate, however, that the problems confronting Argentina in the 1960s (e.g., economic stagnation, disastrous inflation, political states of seige, and military repression) must have fed a disposition to seek fundamentally new economic and political arrangements. In that context, especially for activist middle-class youth, there must have been special appeal in a revolutionary ideology that combined a powerful world view and an attractive idealism on the one hand and a "scientific" rejection of discredited institutions and practices on the other.

Note

We wish to thank Jeane Kirkpatrick for her assistance in obtaining the data upon which this article is based.

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BOOK REVIEWS

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REVIEW ESSAYS

Britain against Itself: The Political Contradictions of Collectivism. By Samuel H. Beer (New York: Norton, 1982. xvi, 231p. $18.95, cloth; $5.95, paper).

The British Prime Minister. Edited by Anthony King (Durham: Duke University Press, 2d ed., 1985. xiv, 275p. $27.50, cloth; $12.95, paper).

Reagan, Thatcher, and the Politics of Decline. By Joel Krieger (New York: Oxford University Press, 1986. xv, 320p. $29.95, cloth; $11.95, paper).

Dilemmas of Change in British Politics. Edited by Donley T. Studlar and Jerold L. Waltman (Jackson: University Press of Mississippi, 1984. xiv, 251p. $17.50, cloth).

The study of British politics is in a remarkably creative period of self-conscious rethinking and reappraisal. As the old verities of British politics have crumbled over the last two decades, political scientists have had to rebuild their understanding of a rapidly changing political order. The years since the late 1960s have been traumatic ones for several western democracies, but no western democracy has experienced a greater shift in self-perception than Britain. In the 1950s Britain was regarded as a paragon of political consensus and effective government; by the 1970s Britain had been transformed into a negative model of political discord and pluralistic stagnation.

Our understanding of British politics must encompass changes reaching into every sphere of political life: decreasing political trust, deference, and consensus; the diminishing salience of class alongside increasingly unpredictable patterns of voting; a rapid decline of Conservative and Labour duopoly of electoral support and a loosening of their grip on parliamentary representation; an intensification of ideological dispute between, and especially within, the major parties; mistrust and bitterness in industrial relations; a nationalist challenge to the unitary state; and a unitary assault on local government autonomy. These developments have taken place at a time of fundamental reorientation of policy priorities and insistent demands for major institutional change. The vaunted capacity of the British to avoid hard choices by finding a path of least resistance now appears more a liability than a strength.

How do we make cumulative sense of these developments? What are their underlying causes? What do they portend for liberal democracy in Britain?

These questions are met head on by Samuel Beer in *Britain against Itself*, published in 1982, but not previously reviewed in *APSR*. Beer's thesis is, at bottom, a cultural one. He argues that the fundamental source of the transformation of the British polity is an unprecedented decline in the civic culture, the balance of modern participatory orientations and traditional deference, civility, and trust that underpinned political stability and governmental effectiveness in collectivist Britain. In thrusting political culture to the foreground of his explanation, Beer is continuing a line of ideational analysis that he developed in his immensely influential book, *British Politics in the Collectivist Age*. The result is an original and challenging analysis of what Beer calls the "new populism," an assertion of values sympathetic to radical individualism, popular participation, and decentralization and antagonistic to traditional hierarchy and authority. Beer traces the origins of these values in the New Left and mass culture of the 1960s and explores their expression in electoral behavior, intraparty conflicts, back bench assertiveness in the House of Commons, and shop floor activism in industrial relations.

By relating change in British politics to a common source—the new populism—Beer brings a satisfying measure of coherence to his explanation. At the same time, he avoids reductionism by emphasizing the unique implications of the new populism in each of the spheres he describes. But the reader can hardly avoid the conclusion that populism means very different things to different people. For those on the left of the Labour party, it is expressed in demands for the democratization of government, the Labour party, and industry. For neoliberals in the Conservative party, populism underlies the attack on traditional

Tory commitments to *noblesse oblige* and hierarchy. Within the mass culture of the 1960s, populism is seen in the Edenic imagery and romantic individualism of popular rock music. But if we cannot distill the consequences of the "new populism" into a consistent formula, this is more an inevitable result of the complex cultural analysis that Beer is engaged in than it is a sign of muddled thinking. To understand the genesis of political conceptions about society—which is ultimately Beer's project—we are compelled to rely more on interpretation than demonstration. In the end, the litmus test of the analysis is not whether it meets formalistic methodological criteria but how it sheds light on the course of cultural tension and change in Britain, and on this score Beer's study is admirably suggestive.

The most original—and contested—aspect of Beer's thesis is his attempt to show that the origins of the new populism are to be found in the counterculture of the 1960s, and particularly in the lyrics of the Beatles' songs. The Beatles were, he suggests, the "'unacknowledged legislators' of Britain's populist revolt." They did not come out for participation or decentralization in so many words, but the influential message of their songs was one of intense individualism and antihierarchy, romantic disavowal of customary social constraints, an appeal to universal human feelings and a pervasive "sense of place." To this writer, Beer's discussion of the Beatles' lyrics is subtle and revealing; other commentators have complained that the evidence Beer marshals is impressionistic and unconvincing. Whatever our view of the validity of Beer's thesis, he has uncovered an immensely important topic. At the very least, this book should precipitate further investigation of the effects of mass culture on political culture. Beer's argument rests mainly on textual analysis; there is a complementary need for systematic study of the ways in which such texts were actually experienced and interpreted.

Beer is virtually alone among established scholars of British politics to investigate seriously the roots of cultural and political change in the popular music of the 1960s. Casting aside the fear of ridicule by those who intone that an analysis of the Beatles' lyrics "cannot be taken seriously," Beer has, once again, extended our understanding of the sources of political culture. In this respect his work is complementary to that of Richard Hoggart or Raymond Williams on popular literature, language, and thought.

If the civic culture has indeed collapsed, as Beer claims, the consequences for liberal democracy are likely to be serious. In their classic comparative study of political culture, Almond and Verba argued that a viable and stable democracy was possible only if the participant-rationalist model of citizenship was balanced with passivity, trust, and deference to authority. Yet for all the pluralist stagnation that Beer diagnoses in Britain, the foundations of liberal democracy seem secure. When we view Britain's present troubles from the perspective of the twentieth century as a whole, the recent urban riots, industrial disputes, intraparty conflicts, and proposals for constitutional change are hardly new in scale or intensity. Unfortunately, we lack a sufficiently distant baseline for diagnosing the severity of recent cultural change. The initial use of systematic techniques for analyzing political culture coincided with an extraordinary period of stability and prosperity in the late 1950s. Those balmy days have gone, perhaps forever, and it is not really surprising that this is reflected in a deterioration of the civic culture, but whether this amounts to an historic collapse must remain an open question. If trust and confidence in political institutions are at all influenced by their perceived performance, the 1950s may just as likely represent an unprecedented high point for the civic culture as the 1970s an historic low.

Alongside this cultural analysis, Beer poses complementary structural and class-based explanations for the failure of the political process in Britain. In his discussion of the structure of decision making, Beer's unifying theme is the prisoner's dilemma, where rational strategy on the part of isolated actors leads to outcomes where all are worse off. Beer diagnoses this situation in several fields of political decision making, from the self-defeating competition for resources among trade unions and subsidized functional groups to bidding between parties for electoral support and the predisposition of government ministers to defend their own departmental budgets irrespective of broader policy priorities. Beyond the varieties of immobilism at different levels of British politics, Beer argues that there is a common inner logic of decisional frustration. In each case, the sheer number of decision makers, their mutual isolation and lack of trust make it difficult for them to cooperate in achieving collective benefits. None of these individuals or groups, according to this argument, will have any material incentive to restrain their economic demands from "the commons" if they believe that their own restraint will not be reciprocated by those in a similar position.

Beer argues persuasively that the prisoner's dilemma has become a more accurate model of political decision making in Britain as nonmaterial incentives for self-restraint, based in traditional social norms and class solidarities, have lost ground. The rise of populism has undermined hierarchical and organic values; class decomposition has intensified group rivalries; the sheer number of interest groups has greatly increased. On the one side, the capacity of the collectivist state to aggregate interests has diminished; on the other, interest groups themselves have become more fragmented. Functional interest groups, voters, and parties find themselves imprisoned in a system of self-defeating competition that punishes isolated attempts to cooperate.

Beer draws on the logic of the prisoner's dilemma to produce an elegant account of Britain's political problems. It also confirms his conclusion that the solution to Britain's political problems must involve the restoration of nonmaterial incentives for creating common benefits; in short, there must be a restoration of political trust. Beer views the Social Democratic party as the best agent for this, because it has the potential to adapt the disintegrative force of the new populism to creative reform and the renewal of legitimacy in Britain's political institutions.

One of the most outstanding virtues of *Britain against Itself* is that it encompasses structural, social, and cultural factors within a coherent explanatory framework. Beer recognizes that no one of these factors can account for the failure of political decision making in Britain, and he manages to combine them in convincing fashion. Each, Beer argues, may be seen as an outcome of the contradictions of collectivism. The new populism, with its emphasis on radical individualism and participatory democracy, is a reaction against the faceless impersonality and functionality of collective representation. Class decomposition is intensified by the preponderance of functional groups rather than classes as a channel for expressing economic demands, and pluralistic stagnation is exacerbated both by Hobbesian competition among numerous producer groups within the managed economy and the lack of any self-imposed budgetary constraints among individual voters in their response to party promises. Implicit in this analytical framework is the sensible supposition that we must combine cultural analysis with structural and sociological approaches to gain an open-textured understanding of political change in Britain.

Virtually absent from the picture, however, is any sustained discussion of the role of economic factors, an omission all the more surprising in light of the dramatic relataive decline of Britain's economic fortunes in these decades. Beer explains in the introduction that his concern is with the way in which decisions are made rather than the substance of the decisions themselves. In neglecting public policy, Beer is true to his professed aim, to restrict his analysis to the political process itself. But the question we may ask is whether it is meaningful to analyze the process of political decision making in isolation from its performance, or, to put it epigrammatically, whether in this case it makes sense to sever the "how" from the "who gets what" of politics. Beer's determination to focus on the one and ignore the other leads him to some one-sided conceptualizations. For example, political distrust is treated as the lack of correspondence between the way public choices are made and the normative standards regarding them. This is sensible as far as it goes, but especially in the case of Britain, it seems necessary to make conceptual space for political distrust that arises in the tension between expectations of government and its performance.

The transparent failures of economic policy and the realization that they were undermining the postwar consensus profoundly influenced British politics in the 1970s. The remedies that were sought in these years shaped the process as well as the content of policy. Sometimes the effects were unintended. For example, incomes policy, which was designed to reduce wage inflation, actually intensified wage competition and extended the scope of state involvement in the labor market, while diminishing its legitimacy. Incomes policy and pluralist stagnation, policy outcome and the political process, were so closely intertwined that we can usefully think of them as existing in a dialectical relationship, each acting on the other. Mrs. Thatcher's policy of excluding union leaders from government councils, avoiding any explicit or compulsory incomes policy, and relying instead on very high unemployment, seems to be a conscious response to the pluralist stagnation that was diagnosed in the 1970s. Here, then, is an instance in which the causality is reversed; the political process is the object, and economic policy becomes the lever.

One must tread cautiously in criticizing a pathbreaking book because of its omissions. It is an irony of our enterprise that the more precise, coherent, and elegant an explanation, the more it invites criticism as being incomplete. By any standards, *Britain against Itself* is an acutely conceptualized and superbly written analysis of Britain's recent political problems. Samuel Beer has that rare intellectual ability to distill the essential features of a complex situation into an explanation that makes sense. The result is a book that is as useful for the undergraduate as it is for the specialist in British politics.

In their edited book, *Dilemmas of Change in British Politics,* Donley Studlar and Jerold Waltman have brought together eight previously unpublished articles concerning the experience and future possibilities of reform in British political institutions. Given the breadth of topics covered—Parliament, the civil service, the party system, local government, devolution, the referendum, the European community, and the courts—the contributing authors show a remarkable degree of agreement that British political institutions are overcentralized, exclusionary, and excessively rigid. They follow Samuel Beer in emphasizing the rapid emergence of popular demands for decentralization and greater participation yet find little evidence of institutional adaptation to such demands. Pragmatism, moderation, compromise—values that were once seen as contributing to the vaunted flexibility of the British constitution, are now viewed as leading to *ad hoc* improvisation and immobilism.

With customary wit, Jorgen Rasmussen asks in the title of his chapter if Parliament is revolting but actually presents a historically informed analysis of the representation of constituency interests in the House of Commons and concludes, with considerable force, that the emergence of highly disciplined parties in Britain in the late nineteenth century was and is inimical to the expression of local political diversity. This provides a justification for devolution to elected national assemblies in Scotland and Wales. But Rasmussen also stresses that the two major parties are unrepresentative of constituency interests within England itself, and, to cope with this, suggests a variety of far-reaching reforms including primary elections.

If the problem in constituency representation is party-induced rigidity, that within the civil service is, according to James Christoph, policy drift beyond the control of well-intentioned reformers. Even when institutional reforms have been deliberately planned and implemented, the intended consequences are confounded by external events. As Christoph observes, the Fulton-style reforms of the 1960s were either stifled by civil servants themselves or overshadowed by broader developments. The most important of these was the expansion of unionism into the middle and higher levels of the civil service, as public workers struggled against compulsory incomes policies and cash limits.

The theme of superficial change in an inflexible political system is also taken up by Douglas Ashford and Jerold Waltman. Ashford documents the follies of institutional reform at the local level and the self-centered and ultimately self-defeating partisanship of both Labour and Conservative governments. Lost in all of this, according to Ashford's astringent commentary, is any sense of the responsibilities of central government to local interests and aspirations.

Waltman finds that since the 1960s there has been a slight broadening of the scope of judicial review of administrative action in response to the arguments of some leading activists, notably Lord Denning, Master of the Rolls until 1982. But Waltman also recognizes that the enactment of a Bill of Rights establishing the rule of law as a curb on Parliament faces enormous political obstacles.

Chapters on the party system, devolution, the referendum, and the European community by Kenneth Wald, Norman Furniss, Harry Lazer, and Hans Michelman, respectively, emphasize fundamental continuity against a background of changing attitudes and institutional tinkering. The picture we are left with is one of archaic, centralized, and inflexible institutions unable to adjust to changing demands. A variety of reasons are suggested for this, but, as Donley Studlar points out in the conclusion to this volume, the most consistently cited culprits are the major parties and the centralizing tendencies of cabinet government.

Such criticisms are anything but new. Despite long-cherished notions of incrementalism and consensus as the guiding principles of British political development from the nineteenth century, political reform proceeded through a series of unpredictable, dissentient, jarring episodes motivated by fundamental critiques of political institutions and the hint—and sometimes the practice—of political violence. In the nineteenth and twentieth centuries, British institutions were not stirred around by military defeat, civil war, or revolution. Reform has had to come by breaking in or pushing very hard from the outside.

Once, not too long ago, U.S. observers were singing the praises of the British polity. Now they are among its harshest critics. Most of the essays in this collection are constructive. They begin with the presumption that more decentralization and more participation would be good and go from here to analyze the limitations of British institutions and occasionally suggest remedies. Few nowadays would take issue with the value of more decentralization and participation, but before setting out on the reconstitution of the British polity it would be useful to have some firm idea of the possible consequences of this for the regime itself. Moreover, unless we believe that all good things go together, we are faced with the prospect of some trade-off among values.

These underlying issues are not taken up in this volume, and the reader will have to turn elsewhere for sustained discussion of the perspective that infuses individual chapters. This aside, the essays in this volume set a high standard of informed commentary and analysis, and are another reminder that U.S. scholarship on British politics is as lively and confident—and critical—as ever.

A critical perspective on recent developments infuses Joel Krieger's newly published book. In *Reagan, Thatcher, and the Politics of Decline,* he traces the fall of the Keynesian welfare consensus in Britain and the United States and passionately denounces the neoconservative nostrums that have guided governments in the 1980s. Krieger places substance above method, and this book presents a readable and refreshingly committed overview of the changing principles of public policy over the last two decades.

Krieger argues that the postwar consensus on the benefits of government intervention to harmonize interests, regulate the market economy, and finance universalistic welfare standards was doomed by economic recession and geopolitical decline. The economic crises of the 1970s severely weakened labor and fragmented the political constituencies of traditional European social democracy and U.S. New Deal liberalism. At the same time, the economic principles of the Keynesian postwar consensus were discredited, leaving its defenders exposed to radical critiques from the Left and, more ominously, from the new Right. Geopolitical decline exacerbated these challenges. Britain experimented with the status of a lowly second-rate power and the United States lost some of its huge comparative advantage. In both societies room for economic maneuver diminished as the scope for conservative appeals to national symbols increased.

The story is, of course, a familiar one. But Krieger is able to hold the reader's interest by subtly weaving detailed narrative into the larger picture. Descriptions of key events, such as President Carter's "moral equivalent of war" speech and Prime Minister Callaghan's defiant speech in defense of continued incomes policy at the miners' gala, serve as lightning rods for broad generalizations. Krieger artfully shifts back and forth between the particular and the general, combining a sense of perspective, some sharp wit, and a fine eye for detail.

Krieger's condemnation of neoconservatism is focused on the social policies that it entails. Reagan and Thatcher, he argues, have picked up the pieces of the shattered Keynesian welfare state, and have put them together in a radically different fashion, emphasizing deintegrative strategies in place of consensus, political exclusion in place of participation, executive autonomy in place of accountability, and a broad retreat from universalistic welfare measures except where they serve a particular group's self-interest. These moral criticisms are powerful, but they apply more to the intentions of these governments than to their achievements. Thatcher and Reagan may have wished to break the back of the welfare state, but, as Krieger himself recognizes, they have been mightily constrained by public opinion. Both have attempted to reduce the role of unions in policy making, and here they have largely succeeded. But neither government is really beyond the realm of interest group politics and popular pressure, as Krieger makes them out to be, or perhaps as they themselves would like to be.

Krieger's argument sidesteps rather than confronts the central claim of the neoconservative program that "there is no alternative" (TINA). Neoconservatives did not argue that high rates of unemployment or reduced welfare were good in themselves, only that these were entailed in dealing with other, more fundamental, problems. A persuasive critique of neoconservative policies would have to examine the logic of this choice and the availability of alternative courses of action. The moral censure of Thatcher's social policies would be much strengthened by examining the assumptions and the practice of the government's economic strategy, especially during the wasteland years of 1979 to 1982. By arguing that neoconservative policies illustrate the dilemma of combining capitalism and democracy, Krieger appears to accept by default the view that there really was no viable alternative.

The comparison between Britain and the United States is an interesting one and allows Krieger to take analytical advantage of some close parallels. In both countries, economic performance in the 1970s failed to meet even modest expectations, both countries have seen a downward slide in their relative political and economic standing in the world, and the victories of radical neoconservative leaders were barely 18 months apart. Krieger discusses the larger implications of each of these insightfully, without forcing unique features of Britain and the United States into a theoretical straightjacket. But the thesis linking these phenomena together is overly narrow and elusive.

Krieger interprets geopolitical decline largely in terms of empire, arguing that imperial decline undermines consensus on social reform and welfare provision. This follows, conversely, from the general argument that the growth of empire—witness Bismarck and Disraeli—is strategically linked to the growth of the welfare state. The more closely one examines this line of reasoning the more problematic it becomes. Far too much intervenes between empire building and social welfare policies for there to be a straightforward relationship between them. Once we test the hypothesis against the experience of other colonial powers in Western Europe (e.g., France and Belgium, both welfare laggards) and countries that had few exploitable colonies (e.g., the Scandinavian societies), it becomes clear that the role of empire is ambiguous at best. Other factors seem more important, including processes of diffusion, the strength of middle-class liberalism, and the timing and character of working-class political mobilization.

Whether or not the hypothesized relationship between empire building and welfarism has any validity for the nineteenth and early twentieth centuries, it is highly doubtful for the post–Second World War period. After all, in Western Europe the most decisive *advances* of the welfare state coincided with unparalleled *loss* of empire and with a generalized geopolitical *deficit* compared to the prewar era. In Britain, Attlee's great reforming Labour government created the basis of the modern welfare state as it presided over an unprecedented breakup of empire.

Krieger dates the decline of the postwar consensus in Britain and the United States with the economic shocks following the oil crisis of 1973–74. Actually, as Samuel Beer points out in *Britain against Itself*, cracks began to appear in the mid-1960s with rising inflationary pressures, intensification of industrial conflict against the background of divisive incomes policies, and shop floor and student radicalism. In the United States we can only speak of a Keynesian welfare state in the most minimal terms, and, to the extent that it existed, it was challenged in the 1960s on several political fronts, by Goldwater and others within the Republican party and by rebellious students. In both countries, the political rot in the postwar consensus set in before the economic crises of the 1970s.

The comparison between Britain and the United States is an instructive one, and perhaps it can be taken further. Britain and the United States share much more than experience of economic recession or political decline, and some of their further commonalities appear relevant to the strength of neo-laissez-faire on the political right. Historically, laissez-faire has been stronger in Britain and the United States, both as a doctrine and in practice, than in any other societies in the world. In the United States support for laissez-faire was linked to individualism and antistatism, values that have deep roots in U.S. culture. In Britain, the Conservative party was a pillar of the establishment and traditionally emphasized pragmatism, patriachialism, and *noblesse oblige*. But in the twentieth century the party was also influenced by a strain of classical liberal thought reflected, for example, in the doctrinaire opposition to state planning voiced by leading Conservatives including Enoch Powell, Peter Birch, and Peter Thorneycroft in the late 1950s. Thus, in explaining the rise of neo-laissez-faire, it seems sensible to refer not only to the decline of the Keynesian welfare consensus, but also to the predispositions of significant groupings within the Conservative and Republican parties. Some party-political factors may also have played a role. Unlike Christian Democratic parties in central and southern Europe, the Conservative and Republican parties are free of traditional Catholic antiliberal, anticapitalist influence, and unlike the Protestant parties of the Right in northern Europe, they do not need to compromise in coalition governments with Centrist parties that are unsympathetic to laissez-faire.

Although the analysis presented in *Reagan, Thatcher, and the Politics of Decline* is sometimes narrow and methodologically unsophisticated, the commentary on the fall of the Keynesian welfare consensus and the victory of the radical Right holds the reader's attention by continually searching for the larger meaning of specific political episodes. *Reagan, Thatcher, and the Politics of Decline* is a book that will further stimulate debate on the causes and consequences of neoconservative policies in Britain and the United States. Joel Krieger stakes out his own position boldly, elaborating a critique of new right governance that is both emotionally charged and intellectually stimulating.

In a period when the prime minister lends her name not only to the present government, but also to the whole range of its policies, the institution of the prime ministership looms particularly large in any evaluation of British politics. In the second edition of *The British Prime Minister*, Anthony King has brought together a colorful collection of essays written by one former prime minister (Harold Wilson), several who have observed the prime ministership at close range (Tony Benn, Kenneth Berrill, Richard Crossman, and Bernard Donoughue), and three scholars in the field (George Jones, Richard Neustadt, and King himself). As noted in the introduction, surprisingly little has been written by political scientists on the topic. For those teaching courses on British politics, this book fills that gap nicely.

By focusing on the institution at the summit of British politics and the individuals who have shaped it, this book provides a useful counterpoint to structural analyses of political and economic change. The role of political leadership, personality and, one should add, good fortune, is nowhere so apparent as in the way Thatcher has promoted her own policy goals as prime minister. As Anthony King emphasizes in his chapter on Thatcher's prime ministerial style during her first term, she is unusual and probably unique among prime ministers of this century in coming to office with a policy agenda quite distinct from that of most Conservative members of Parliament or members of her cabinet. Given Thatcher's initial isolation within her own government, the policies that were pursued—for example, privatization, drastic economic retrenchment until 1982, and union-curbing legislation—were extraordinarily dependent on her own determination and acute powers of political calculation and timing. Also, one cannot forget the luck—*fortuna* in Machiavelli's sense is perhaps a better term—of the timing and outcome of the Falklands War, which allowed Thatcher to turn the tide both within the cabinet and in the opinion polls.

Thatcher's tenure as prime minister leaves no doubt about the flexibility of the institution. Quite simply, she has shaped it to her needs as she perceives them, asserting her views from the outset in cabinet meetings, extending her oversight of ministerial decision making, and influencing the promotion of senior civil servants. It is tempting to construe Thatcher's attempt to dominate government as a *post hoc* demonstration of Richard Crossman's thesis regarding the evolution of cabinet government into prime ministerial government. Thatcher has extended the boundaries of prime ministerial power, but for all her ambition to be at the hub of government, she still has had to convince or cajole independently powerful cabinet ministers, who are competitors as well as colleagues. Thatcher has led from the front, but she has also been overruled by the cabinet probably more frequently than any prime minister this century. George Jones's classic article on the prime minister's power remains the more convincing account. If his balanced appraisal of the power of the prime minister is lacking, it is mainly because —writing in 1965—he could not take into account the establishment of the prime minister's own policy unit and more extensive use of personal advisers, the growth of the cabinet office as a further source of independent power, and the increasing delegation of policy making away from the cabinet to its various committees. But recent political change has not been exclusively in the direction of increasing prime ministerial power. Minority government (as from 1976 to 1979) and the rise of back bench assertiveness both reduce the ability of a prime minister to enforce her will in the House of Commons as party leader.

Thatcher's activist prime ministerial style and abrasive personality provide a perfect foil for the more receptive style of Harold Macmillan or the more passive style of Douglas Home. By contrast with Thatcher, Harold Wilson appears even more distinctly as the "great conciliator." A prime minister, in Wilson's words, should be concerned with "face-saving and binding up wounds, intervening also to soften asperites and curb the invocation of personalities." Wilson's essay, along with some others in this book, may be read at two levels, for they reveal as much about the author and his own predilections as about the institution itself. Indeed, as King emphasizes, we must be cautious about analyzing the prime ministership in the abstract; within broad limits the office is what the prime minister makes of it.

Each of the books reviewed here calls for some reinterpretation of contemporary British politics. From their different perspectives these books compel us to reconsider the strength of democratic values, the effectiveness of political decision making, the flexibility of political institutions, the democratic integration of outgroups and minorities, and the concentration of political power in Britain. Britain suffers, we are told, from the decline of its civic culture, pluralistic stagnation, adversary party politics, a loss of geopolitical standing, and excessively centralized, unresponsive, political institutions. Choices have become tougher. In recent years it has become immensely more difficult to blend tradition and modernity in British politics, to combine cabinet government and collectivist bargaining, parliamentary sovereignty and participatory democracy, nationalization and free enterprise, the unitary state and local rights.

But before all disappears in dense gloom, we might do well to maintain a sense of historical perspective. Widespread criticism of British political institutions is by no means new, and has in the past been instrumental to the process of political reform in Britain. The first two great reform acts of the nineteenth century were the product of political conflicts at least as intense as any we have seen over the last two decades. Compared to the 1950s and early 1960s, there has been a sharp increase in political discontent and criticism, and an unambiguous decline in political-economic performance. But the period following the end of the Second World War was itself unusual. The two-decade long conjunction of political consensus, industrial harmony sustained though moderate economic growth, and full employment was as unique in Britain as it was in most other Western European societies.

Many of the criticisms themselves represent changing standards as well as declining performance, and it would be surprising if this were not so. Beyond doubt, the emphasis on economic performance in recent years, and our assumption that government is responsible for it, have colored evaluations of political performance. At the same time, traditional standards have come under attack by those stressing greater participation, decentralization, and quality of life issues. From a longer historical perspective, it is hard to deny that the presumption of stable liberal democracy in Britain has itself helped to shift the grounds of evaluation.

Given the scope of political change in Britain, it is not outlandish to hypothesize that liberal democracy is a far hardier kind of arrangement than writers in the 1950s and 1960s believed it to be. They lived through an era in which stable liberal democracy appeared to rest on a rather precise and unusual set of social, cultural, economic, and political conditions. Whether they looked at the explosive failures of democracy in Europe between the wars or the abortive experience of creating democracy by constitutional transplantation in former colonies, their concerns about the restrictive basis of liberal democracy were confirmed. Britain in the 1980s and 1990s may lead the way in revealing to us the adaptability, inherent toughness, and surprising autonomy of liberal democratic norms under economic decline, political dissent, and institutional inflexibility.

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Political Science: The Science of Politics. Edited by Herbert F. Weisberg (New York: Agathon Press, 1986. xii, 307p. $30.00, cloth; $15.00, paper).

A book invariably begins with a title, and a title presumably conveys a message. Making a title redundant gives an author or editor a strategic advantage. It disarms the would-be reader even before he or she has had a chance to ponder the title's meaning. How can the reader resist redundancy? That, I suppose, is what editor Weisberg had in mind when he inserted the colon (:) between title and subtitle. Regretfully, he does not tell us just what the exact relationship of *political science* to the *science of politics* is, might be, or should be. At least I cannot believe that he equates the two. In that case, he would surely have used the equal sign (=) to denote the message. So much for my initial decoding of the title's meaning and the book's intent.

This, then, is a book about political *Science*. There is no room here for political philosophy, practical politics, descriptive case studies, grand theory, the craft of statesmanship, bringing the state back in, and all the sort of thing that fascinates a good many political scientists. Besides, the editor informs us, "the important scientific work being done in international relations, comparative politics, public policy, and the study of race, gender, and ethnicity issues cannot be considered here," but he assures us that "the in-depth analysis of the state of science in the areas included helps make up for this loss" (p. ix). Weisberg evidently has a sense of humor in all scientific seriousness.

The volume contains "theme papers" delivered at the 1983 annual meeting of the APSA that were intended to "extend the science of politics," and indeed several panels at the meeting "were specifically devoted to assessing the status of science in the study of politics" (p. ix). Weisberg as organizer of the meeting and editor of the papers acknowledges that "no attempt was made to define or limit the meaning of the theme," that "different schools and scholars have different interpretations of science in the study of politics," and that "diversity is important to maintain" (p. 3). There is more such genuflection to scientific tolerance and pluralism, but I suspect Weisberg had his tongue in cheek when writing this. If, even within the relatively narrow bounds of what a *science of politics*—in contrast to *political science*—is about, each author is free to interpret science differently, I wonder whether it makes any sense at all to make either *science* or *Science* the criterion for appraising a "discipline" that is as notoriously undisciplined as political science appears to be.

The trouble with making *science* a theme in assessing the study of politics is that, in field, laboratory, library, or even armchair, the scholar studying a *problem* does not *begin* his or her work by worrying about science, in whatever disguise. Rather, he or she tries to solve a problem of interest in the most plausible way possible, by whatever means. I don't see why, *in the end*, we should worry about the scientific status of research once it has been completed and reported, there evidently not being a single road to salvation, protestations about science notwithstanding. As the anthropologist Clifford Geertz (1973, 5) once wrote, "If you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do."

I am happy to report, therefore, that several of the papers assembled by Weisberg meet Geertz's criterion and allow us to peek at what their authors as practitioners are up to. I would place among them Shepsle's "Institutional Equilibrium and Equilibrium Institutions," Dodd's "The Cycles of Legislative Change," Miller and Moe's "The Positive Theory of Hierarchies," and Franklin and Jackson's "Structural Estimation with Limited Variables." These four papers try to cope with very different problems and do not worry about the scientific status of the work involved but give us some insight into how their author-practitioners go about it.

There is a second set of papers, more self-consciously reflective and in part prescriptive but clearly informed by research experience. Their authors are more concerned with what one calls the "state of the art" in a given field of inquiry than with the state of science in some Platonic heaven. I would include here MacRae's "The Science of Politics and Its Limits" (its title notwithstanding), Niemi's "The Dynamics of Public Opinion," and Weisberg's own "Model Choice in Political Science."

A somewhat different genre is represented by a set of papers that are more or less comprehensive assessments of entire subfields, and their authors seem to worry a good deal about the scientific status of particular contributions in these fields. They are also more or less critical in terms of some presumably self-consistent criteria of science, but as the criteria tend to differ from one subfield to the next it is difficult to compare their scientific status. Fortunately, these papers come off the word processors of talented research practitioners who, in fact, tell us more about their own state of mind than the state of their subfield. I list among them Rockman's "Presidential and Executive Studies," Gibson's "The Social Science of Judicial Politics," and Beck's "Choice, Context, and Consequences: Beaten and Unbeaten Paths toward a Science of Electoral Behavior."

Finally, there is a paper that stands by itself, Kramer's "Political Science as Science." It is an unadulterated statement of SCIENCE. I wish I had the space to unravel all of Kramer's views; but this would only get me into the philosophy of science that, I prefer to think, is the business of the philosophers and not of the scientists. What these views come down to is that Kramer is a believer in deductive modeling and a reductionist methodology reminiscent of the Vienna Circle's logical positivism or logical empiricism and, therefore, just the opposite of what Geertz recommends as the pragmatic criterion for understanding what a science is. I want to say only two things in addition. If, as Kramer suggests, Milton Friedman's *A Theory of the Consumption Function* "could serve as a useful paradigm for political science: a precise, simple, but powerful explanatory hypothesis, rich in empirical implications" (p. 22), first, only Allah himself will save us from this particular scientific dispensation; and second, any number of sciences—from astronomy to chemistry or geology, including political science, might as well close up shop. But, then, I don't believe that even physicists and mathematicians go about their work in the way that Kramer recommends (just read the confessions of the mathematician Jacques Hadamard [1949]). There is a metaphor for one method as scientific as any other: trial and error. The trouble is not with the method but that scientists, including those who cultivate the gardens of politics, rarely tell us what they tried and how they erred.

In general, however, the contributions to the Weisberg symposium are considerably less purist than the theme they were commissioned to address at first leads one to assume. This is not to say that I share Weisberg's politically congenial disciplinary pluralism, if he means it. Weisberg, it seems to me, confuses what we should expect from theorizing with what we should expect from (forgive the word) methodizing. Insofar as political science aspires to scientific status, it thrives on the competition of theories but also suffers from the fact that, unlike the case of "more scientific" fields, one theory never drives another out of the market. It may do so for a time, but that is more due to fad or fashion than proof or disproof. That is the reason I have never accepted Thomas Kuhn's (1962) image of scientific development as pertinent to the social sciences. Yet this image is refracted in much recent discourse about the science of politics, in this volume and elsewhere. All the talk about new paradigms notwithstanding, political science in its U.S. version has never been anything but excruciatingly normal. It is our civic and secular religion, even in its neomarxist clothings. What has changed, however, is its methods. I respect Achen's (1983) critical stance in the matter, but I believe that in the last 30 years or so methodological change and advance have been the most remarkable aspect of development in political science. When Beck, for instance, unashamedly refers to the study of electoral behavior as "the most scientific" field in political science (p. 241), he clearly thinks more of changes in method than in theory. Theories change in the sense that they are extended, refined, explicated, and so on, but one theory is never jettisoned forever in favor of another. Old methods are in fact discarded, however, and replaced by new when and if the new prove to be superior to the old. I have a hunch that most of the authors have this in mind when they appraise the scientific status of various subfields. Whereas theories divide and should divide, methods are or should be the bond. When all is said and done, the validity of a theory and the reliability of theory-driven findings can only be ascertained if there is a modicum of agreement on methods.

By *method* I do not simply mean, of course, *technique.* I include the formulation of theories and hypotheses, the criteria of evidence and inference, and what Landau (1972) so felicitously called "due process of inquiry." Most of the papers in Weisberg's symposium deal with these matters in one way or another and more or less successfully. The problems of method, especially the choice of methods, is not a matter of taste, as I think the choice of theory often is, but a matter of persuasion. We expect the researcher to persuade us, and we want to be persuaded, that the ways in which he does business, so to speak, are appropriate. Let me illustrate this by juxtaposing the papers of Kramer and MacRae. The former sees "model building" as the primary task. As theorizing in the deductive mode, no matter how rigorous it appears to be, is rather intuitive and permits many alternatives, I would expect the sophisticated modeler to take a rather broad and tolerant view of the scientific enterprise. But Kramer's position is rigid. MacRae, on the other hand, proposes a most demanding criterion for scientific status—prediction. One might expect him, therefore, to take the narrow view that a science is as good as its predictions are good—and on this criterion the social sciences, including political science, are certainly flawed. Yet MacRae entertains a perspicacious view of the enterprise. He concedes the contributions that phenomenology or hermeneutics can make to our understanding of politics that Kramer would in all likelihood read out of a science of politics. I am more persuaded by MacRae than by Kramer.

It is similarly convenient to juxtapose the essays of Dodd and Shepsle. Dodd constructs a "cyclical theory" of institutional change in legislatures that is a highly stylized and empirically suspect description of Congress in the early 1970s. (As Dodd himself points out, "the theory should fit this case, if no other, since it was created in response to these reforms" [p. 99].) Dodd's method is one of extrapolation of micromotivational tendencies to the macroorganizational level. I am not persuaded by this method of theory construction. By way of contrast, there is Shepsle's effort at formulating a "new institutionalism" that, in effect, restores politically-inspired sanity to economic hallucination. Shepsle retains what I think is best in rational choice theorizing but makes adaptations to the real world of legislative praxis that, it seems to me, are plausible and, above all, falsifiable. I am persuaded.

A third set of papers conveniently juxtaposed are Miller and Moe's and of Franklin and Jackson's. Although their topics are very different, both are commendably pedagogic. Both are concerned with capturing reality, one by empirically germane modeling, the other demonstrating a statistical "observation model" that presumably accounts for more than the usual regression models. Both make a common assumption: that what we observe is not the same thing as what may be out there in reality. It is quite usual for analysts to go off in theory or method and then, having reached dead end, to tell us, in what is called "ordinary language," where they have been or where they are now. This procedure, of course, simply confuses the real world with ordinary language. As the philosopher Carl Hempel (1965) instructed us some time ago, what we have when we speak of "facts" or "data" are at best "observational reports." Both theory and method ought to be sensitive to the contingency that reality and the reports we have of it are not the same thing. Both papers, each in its own way, make Hempel's argument. They are persuasive and of the kind on which to cut one's teeth.

I tread with caution in comparing two other papers, essentially surveys of their respective subfields by Rockman and Gibson. Both authors clearly took seriously the instruction to assess the scientific status of executive and judicial politics studies. Not surprisingly, each paper comes out with different conclusions, largely, I suspect, because the two fields face different empirical problems. It is the familiar "few cases, many variables" syndrome in the study of the U.S. presidency (the core of Rockman's essay), and the converse "many cases, few variables" syndrome in the study of judicial politics. But if the former makes Rockman somewhat cautious and rather gentle (for much of what he writes about is more soft thinking than hardnosed research), the latter makes Gibson rather reckless and critical in appraising often mindless empirical research. Nevertheless, I should note that both authors give due recognition to at least one piece of research, respectively, that seems to meet their criteria of good science in these subfields—in the field of presidential politics the work of John Kessel (1975), and in the field of judicial politics the work of Woodford Howard (1981).

There are what I would call epidemics of consciousness that from time to time sweep across political science. More often than not, they are as quickly forgotten as last Sunday's sermon. For some years now, the word *context* has been as fashionable in article titles as genuine contextual analysis has been missing (but for the most recent exception see Huckfeldt 1986). In general, the specification of context and its treatment as either an exogenous or endogenous *variable* remains vague. Unless contexts are in fact varied, it is doubtful that the contextual criterion has been satisfied. For this purpose, relevant data are often simply not available. So it is with "dynamic analysis," a notion that covers a multitude of possibilities. As one turns to Beck's long and lucid explication of what he calls the "Michigan Model" of electoral studies, the broad range of contextual and dynamic problems in the analysis of political behavior becomes apparent. The paper is worth reading on that accounts alone, even if one is not interested in voting behavior as such.

Beck makes many suggestive observations, but I must limit myself to a general comment. I am prepared to accept Beck's notion that the Michigan or Michigan data-based voting studies are prototypical of what good political science can be. Given this bias, I draw this lesson from the exemplar: the fact that a field becomes "more scientific" does not mean that there will be fewer controversies as to theory or finding (see Niemi and Weisberg 1976). On the contrary, as data making and analysis improve and follow different paths, the field's findings become more circumspect and tentative. This may appear to be a paradox, but it is not. If the task of science is to reduce ignorance rather than assert some truth posited in a theory's premises, the evolution of the National Election Studies gives warrant to the field's status as "the most scientific" in political science.

Curiously, a false sense of ambivalence seems to pervade Beck's treatment. I could cite several examples, but one must suffice. One reads, for instance, that "the foundations of the Michigan Model are shaken by these results" (occasioned by nonrecursive modeling), but this comes "at the expense of the considerable parsimony and theoretical strength of the Michigan formulation" (p. 260). I think this false ambivalence has two sources. First, Beck flirts on and off with the Kuhnian notion of "scientific paradigms" but cannot make up his mind whether Kuhn's theory of scientific development is appropriate in the case of the voting studies (as I suggested earlier that it is not). And second, Beck hypostatizes the "funnel of causality" metaphor presented in *The American Voter* into a "model," which, I believe, it was not intended to be. It was a graphic device to bring some order into the vast array of factors presumably influencing the vote. As I noted in reviewing the book 25 years ago, "most of the empirical analyses proceed without the benefit of the 'funnel of causality,' and, perhaps more important, derive from theoretical propositions quite unrelated to it" (Eulau 1960). If there is a Michigan Model, then, it is considerably more sophisticated and complex than the funnel notion suggests. In fact, I find little in the revisionist literature, of which Beck makes a great deal, that cannot also be found in the original Michigan research or its later elaboration. This is not to denigrate the work of those who descended or dissented from what they wrongly conceived to be the Michigan Model. It is to suggest that it is wrong to confuse particular emphases of the Michigan research in a particular temporal context with a single model that, presumably, made a universally valid claim to some Truth.

In conclusion, a general observation on such an effort as that represented in this (admittedly interesting) volume: How wise is it for an APSA program committee to commission a set of papers that presume to be thematic (in this case, something called the "state of science" in political science) but that turn out to be either something more or something less. There is, it seems to me, something inherently artificial in such endeavor. It is a kind of imposed scholarship from the top that has surface plausibility but does not ring true. Our national (or regional) meetings ought to reflect what scholars are in fact doing rather than what particular convention organizers in any given year think they should be concerned with. This is the reason why, among the papers in this book, I find those reporting on their authors' ongoing work to be by far the most stimulating, regardless of whether I agree with their messages or whether they "extend the science of politics."

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POLITICAL THEORY

The Moral Life of Children. By Robert Coles (Boston: Atlantic Monthly Press, 1986. 302p. $19.95).

The Political Life of Children. By Robert Coles (Boston: Atlantic Monthly Press, 1986. 341p. $19.95).

Political socialization, neglected by political science, may yet be salvaged by the impressive efforts from other disciplines. I have previously noted in these pages that cognitive psychologists examining social and moral development have contributed excellent insights questioning many standard assumptions on the evolution of political awareness. Now, from a divergent theoretical perspective, Robert Coles has appeared with two complementary overviews not only of the fieldwork reported in his magisterial five-volume set, *Children of Crisis*, but also of the research subsequently pursued around the world.

At first glance, Coles's books might seem mystifying to political scientists. They are discursive, at times even novelistic; hypotheses are scarce; and the children's own words are accorded seemingly as much space as Coles's analysis, itself almost unsettlingly candid about his occasional unwillingness (or inability) to reach definitive conclusions. Yet it is precisely Coles's unusual approach and unfamiliar method that make these books so essential. Coles is not uninformed of the literature in political socialization; far from it. But by adopting an inductive approach that allows the children's comments themselves to set the books' agendas, Coles has alerted us to many possibilities that political science has scarcely considered at all.

For instance, one of the most central assertions to be derived from these books is that children possess a capacity, even before adolescence, to be thoroughly competent in making moral and political judgments. This conclusion partially comes across through Coles's tone; instead of referring to his subjects as "naive informants" (to use the label of U.S. political socialization), Coles prefers to initiate a dialogue in more egalitarian terms, as in the case of one 12-year-old Cambodian girl whom he calls "an extraordinary teacher of mine" (*Political Life*, 278). But Coles also shows considerable evidence that children do not merely parrot ideas from parents and teachers but instead creatively work these elements into what, without exaggeration, he calls their "political and moral consciousness" (*Political Life*, 303). The position that children can be and are intuitive moralists has been impressively charted by recent examinations of moral development, and thus *The Moral Life of Children* may be the less immediately valuable of the two books. Coles's claims for children's political understanding, on the other hand, have been accorded less currency in political science and deserve greater scrutiny.

To Coles, nationality is a constant in all lives, including those of children. By his psychoanalytic reading, nationality provides not only an outlet for the id or a source of the proscriptions of the superego, but, most intriguingly, a key instrument in the construction of the ego. Coles suggests, presenting pages of evidence from children in numerous locales and regimes, that the institutions, traditions, and symbols of a nation provide clues for children seeking general ways to concoct an identity out of the particulars of their lived experiences. It should be noted that such personal uses of nationalism are not synonymous with what was once dubbed "system support." Nationalism can, of course, be exploited to support the state, but in Coles's examples, it often undermines it, either by allegiance to another political entity (as among Catholic children in Ulster) or its use for cultural and religious institutions against political ones (e.g., in Poland). Nor is nationality an easily avoided fact for any inhabitant of a nation, as Coles persuasively elucidates in his superb chapter on South Africa, relying, as always, not only on interviews but also on exemplary interpretations of children's drawings. To be sure, racial differences are acute: white children, both Afrikaner and English, have fully appropriated racist nationalism; colored children, caught between classifications, are shown to be "the most color-conscious children I've ever met" (*Political Life*, 205) by their extraordinary attention to the skin color in their drawings; black children, loath to portray themselves in their drawings except in a collectivity, express rage at the regimentation of apartheid. Yet, Coles nevertheless concludes, "these boys and girls all think of themselves as South Africans, even dream of themselves as South Africans, no matter what a government says and does and announces it will do" (*Political Life*, 223).

It is difficult to know how generalizable this argument of the near universality of nationalism can be. Clinical psychiatrists are, by training, most interested in the abnormal; Coles has only turned the focus to how normal individuals cope in the midst of unusual and stressful environments and times. While such choices may be apt for noting the resilience of children facing moral and political crises, they may overestimate the extent to which nationalism is a central component of children's identities, political or otherwise. For most of the children studied here (e.g., those from Ulster, Nicaragua, Quebec, Poland and South Africa, and the refugee children of Southeast Asia), nationalism has become a central concern at least in part because of extraordinary political preoccupation with the question of nationality in each society. Whether or not nationality would prove to be a crucial component of identity in less dramatic circumstances is only briefly touched upon in the chapter on Brazil and the United States, where Coles sees nationalism as severely refracted by class. But the discussion is too cursory to allow us adequately to assess Coles's point on the last page, "A nation's politics becomes a child's everyday psychology" (*Political Life*, 310).

Still, the contribution of these two books may not rest on how definitive Coles's conclusions are. His service may be greatest in spotlighting aspects of the child's relationship to politics and morality that have thus far remained insufficiently noted. Instead of pointing to "agents of political socialization," he implicitly urges the reader to emphasize children's personal experience in the immediate surroundings that they often share with those "agents," most particularly the influences of religion and class. Instead of automatically assuming that children are in need of civic and moral training, he favors methods to assess less what children fail to understand than what they grasp on their own terms. And instead of looking for the uses that politics has for imbuing system-supportive behavior in children, he pinpoints the potential uses children have for politics in helping to create their identities. All three of these implications are significant departures from our standard assumptions in political science and alone would make these books worth reading. But with Coles's sophisticated command of psychoanalytic theory and the humaneness and beauty of his prose, they attain the status of must reading. Coles, to be sure, may not be able to provide us with solid answers on the political and moral worlds of children, but he has raised questions that should concern us for years to come.

TIMOTHY E. COOK

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Weberian Sociological Theory. By Randall Collins (Cambridge: Cambridge University Press, 1986. xi, 356p. $42.50, cloth; $14.95, paper).

This readable and well-crafted volume is not so much an exegesis of Weber's thought as an effort to demonstrate its coherence and contemporary relevance. Collins attempts to do both Weber and modern social scientists a favor by correcting or ignoring misconceived or inconsistent aspects of Weber's writings in light of their implicit theoretical unity and their potential for explaining important social phenomena unanticipated by Weber himself. Recognizing that Weber called into question on methodological grounds the possibility of a unified, predictive sociological theory, Collins nonetheless purports to find such a theory at least suggested in the corpus of Weber's work.

This book has substantial virtues that make it profitable reading irrespective of one's methodological commitments. Foremost among these virtues is the emphasis on the importance of political factors in *all* of the diverse social processes treated in Weber's work. For instance, by Collins's analysis, the most important dimension of Weber's mature theory as to the necessary conditions for the rise of capitalism is cultural and political competition. He also argues that Weber differed most from Marx in the conclusion that capitalism could exist indefinitely, given an appropriate political context, and makes a persuasive case that Schumpeter's highly political analysis of the actual workings of market institutions complements Weber's historical analysis of their development much better than the classical views that still influence modern economic theory.

Similarly, Collins attempts to show that religious heresies are occasioned by power struggles within universal religions characterized by a certain organizational structure. He then suggests that the same factors can be seen in secular forms of explicit ideological conflict. Even family structure and the cultural and social status of women were presumed by Weber to be largely the result of military technology and political possibilities. In explicating these arguments, Collins does not hesitate to modify or amplify Weber. Yet he is very familiar with the work of his protagonist, and in most instances it seems to me that Weber lends himself to these modifications, that these arguments can be plausibly claimed as Weberian. This is especially so in that Collins, despite his explicit rejection of Weber's "idealist historicism," for the most part observes Weber's strictures against unicausal reductionism. While politics may be a key factor, it is never a sufficient cause.

There is, however, a very paradoxical exception to this general practice, and that is the treatment of politics itself. Collins argues that internal politics is largely determined by external geopolitics because the ability of a faction to dominate a state largely depends upon its prospects for achieving international victories. Contrary to the importance most commentators on Weber's thought attribute to the concept "legitimacy," it is here reduced to a secondary factor. It is derived from the power-prestige of the state. Indeed, even "nationalism" is no more than a "feeling of awe toward the state," which is the "result of the success of a state in power politics" (p. 154). Although much of the analysis that Collins derives from this starting point is suggestive, it tends to be strained and overly one-sided in its emphasis on the primacy of geopolitics. Furthermore, he violates his own ground rules by insisting that this is the genuine Weber, hitherto misunderstood.

Actually, it is similar to a number of previous interpretations, most notably that of J.

P. Mayer. More importantly, it ignores much of Weber's analysis and many explicit statements. For instance, in *Economy and Society,* Weber asserts that cultural factors may fuse with such political prestige, and when this happens "the naked prestige of 'power' is unavoidably transformed into other special forms of prestige and especially into the idea of the 'nation'" (pt. 2, chap. 9, sec. 5).

Nevertheless, this section of the book is as interesting and suggestive as the others, especially if one views its perspective as one of those one-sided accentuations called ideal types. Collins, however, would not wish to have his arguments so characterized. In any case, he has successfully demonstrated the continuing relevance of Weber's work by offering a number of interesting applications and extensions. But whether this success is attributed to the validity of an implicit general causal theory or to the normative relevance that Weber's concerns still possess for our culture is a question not really addressed in this book.

E. B. PORTIS

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The Nature and Limits of Authority. By Richard T. De George (Lawrence: University Press of Kansas, 1985. viii, 305p. $27.95, cloth; $12.95, paper).

This wide-ranging and insightful book concerns the concept, justification, and limits of authority in various contexts. It is the best to date on the topic. Although it contains lengthy discussions of political authority, it considers nonpolitical authority throughout society. Indeed, a strong point is the showing that consistent anarchists must accept various types of nonpolitical authority. The central claim is that acceptance of authority is the norm, that within limits it is rationally defensible, and that challenges to authority require defense (p. 1).

De George begins with a general working model, which is refined for specific types. An authority stands in a relation of superior to inferior to someone in some realm (p. 14). The fundamental distinction is between *executive* and *nonexecutive* authority. Executive authority is the right or power to do something (p. 17); all other authority is nonexecutive. Unfortunately, the concepts of *right* and *power* are not adequately analyzed, and some arguments seem to use different senses of them. Executive and nonexecutive authority are subdivided by the kinds of response appropriate. Executive authority is divided into *imperative* (evoking obedience) and *performatory* (doing something directly to or for one); nonexecutive authority is divided into *epistemic* (evoking belief) and *exemplary* (evoking limitation). A plethora of other distinctions are made—including *de facto, valid, legitimate, de jure,* and *effective.* These distinctions are then used to analyze authority in the family, voluntary organizations, business, politics, morality, religion, and universities.

The argument for the central thesis is not as explicit or persuasive as desirable; the most explicit defense is given for political authority. Legitimate authority is originally defined as that for which there are good reasons (pp. 19, 142), and the primary justification for any authority is that it benefits those subject to it (pp. 39, 45, 72, 85, 135, 152, 182). This seems to place the burden of persuasion on one who defends authority and thus subverts the main thesis. However, a government is legitimate if most of the people subject to it accept it and think it justifiable (p. 143). This general acceptance of, or consent to, authority switches the presumption to legitimacy so that criticism of authority requires defense (pp. 160–61).

We are left with a tension. Justifiable authority must actually benefit those subject to it (e.g., p. 163); acceptance alone cannot justify authority—only change the presumption. One way to support shifting the presumption of legitimacy is to claim that those subject to authority are the best judges of whether they benefit. This has the advantage of being limited to authority and other matters where benefit to the individuals is at issue, but the disadvantage of resting on an empirically questionable premise. A second way is to claim that if one wants to dissuade persons from believing or accepting something, one must accept the burden of proof. This has the advantage of applying to all practical arguments, but the disadvantage that it has no more to do with authority than anything else.

MICHAEL D. BAYLES

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Law's Empire. By Ronald Dworkin (Cambridge: Harvard University Press, 1986. 470p. $20.00).

What is law? Ronald Dworkin in *Law's Empire* takes up the eternal question of jurisprudence, but to his credit he does not try to nail down an answer. Instead he investigates the status of the question by offering an account both of how judges decide cases and of how they ought to do it. His claim is that judges must interpret first the practices of the legal system of which they are a part and then the moral principles of their society that must inform the law. Only thus can they give the law both "grounding" and "force" in order to legitimate their decisions and the use of coercion by the government. The facts of cases are deliberately ignored, which is unfortunate, since half the terrain of interpretation is thus left out.

Dworkin's version of interpretation is closely tied to a communitarian political theory. Philosophers and judges are to articulate the immanent practices and shared understandings of their communities and not to impose abstract rational and universal standards upon them. Political theorists have long been familiar with Michael Oakeshott's subtle representation of this view. Dworkin regrettably has chosen literary hermeneutics as his model, with all its hostility to causal explanation and reliance on empathy and intuition for understanding social phenomena. Dworkin never asks just how law resembles literary texts for purposes of "reading"; its authoritative character is only mentioned in a footnote and in another context. Nor is it clear why he insists that social scientists can only understand the members, not the practices, of a culture other than their own. To say that they are engaged in an investigation, not a conversation, hardly serves as an argument. There is, in fact, reason to think that we do understand foreign societies and the past of our own. In any case these assumptions justify Dworkin in limiting himself to the practices of his own "flourishing" legal systems of Great Britain and the United States, and in relying on only four, highly unusual, appeal court cases to illustrate his points.

It is easier to know what Dworkin's "law as interpretation" is not than what it is. It is not the "semantic sting" of analytical jurisprudence, used to clarify the existing structure of norms by defining them all exactly. John Austin's and Herbert Hart's jurisprudence cannot account for the real divergences of opinion among judges, for it is based on a simplistic psychology and is devoted to one political end, the security of established expectations, which it fails to examine critically. The undeniable appeal of this flawed theory is due to what Dworkin calls the "conventionalism" of most lawyers. They believe that law is just there, that it is a gapless hierarchy of norms that judges have to find and when it really offers no rule or convention then the judges legislate, but do not tell the public. But since this is not what judges actually do, it is a matter of self-deception or false consciousness. Though they do not know it, judges do interpret the past political decisions that make up the precedents, statutes and political mores of the system, by teasing out their meaning here and now. Interpretation also does not look for an idea of justice. Justice is what anyone in a pluralistic society thinks is best for the community as a whole. And fairness is just impartiality. The two may therefore be at odds, though both are social virtues. So much for John Rawls. Pragmatism, the notion that judges should look only to the future welfare of society, is treated with more respect, though it offers no airtight protection to rights and makes unrealistic moral demands on citizens. However, as policy must enter the decisions of judges, it has its place.

Law, then, is not a system of rules, a pursuit of justice, or a plan for social happiness. It is the interpretation of the social practices inherent in past political decisions and in the principles of the community as they appear to a man and judge of "integrity." Lastly, interpretation is not explanation, or the "external" Marxism and the intellectual muddles of Justice Holmes. In fact the "external" view does not ignore the inner structure or development of law, but tries to integrate it into the political history of the society of which it is a part. The social and political history of law, whether narrative or analytical, is a perfectly plausible enterprise, as is the history of medicine or religion. One may even come to grasp the "natives' point of view." Indeed, Dworkin's brand of hermeneutics is not as acontextual as all that; it reaches out to the moral and political practices of the community at large for its self-understanding. On the whole he also avoids turf fights, and he should have stayed away from this one.

To see what a man of integrity, Hercules the Judge, would do, Dworkin begins with a fiction. Society is to be treated as a personification, like a corporation, and so can be held accountable for its conduct and understood as a single unit. Hercules, in interpreting its legal practices, conceives of respect for statutes, precedents, and consistency as an intellectual virtue. He does not, however, ask about the intention of the authors of a law or even of the Constitution, because it is unknowable. That is also current literary theory. Instead he imputes intentions to them that are likely to put society's practices "in the best light" now, that seem most "attractive" and are in keeping with his "genuine" moral convictions as well as those of other members of the community. There is no way of telling how much consensus would be required for such a judicial performance. Its chief doctrines are limited government, the equal protection of the rights of all members of society, and an equal concern for the fate of all. These are the best part of the political principles of both the legal system and of the polity as a moral unit. What it excludes is slavery and racial discrimination, but, for the rest, its meaning is pretty vague. Most of these moves are at a preinterpretive stage of "law as integrity." The fully interpretive task is one that brings morals embedded in the society to bear on decisions that will have "fraternity" as their aim and principle.

Although it is often repetitive this book is clearly written and will make for lively graduate seminars. Whether one finds it either coherent or persuasive is, as Dworkin would say, a matter of what one happens to think right now.

JUDITH N. SHKLAR

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Critical Theory and Public Life. Edited by John Forester (Cambridge: MIT Press, 1985. vii, 337p. $30.00).

The intention of this highly varied collection of essays is to demonstrate the practical relevance of critical theory in dealing with problems of politics and policy analysis drawn from everyday life. With the exception of Habermas all of the authors are affiliated with U.S. or Canadian universities, yet the collection avoids North American ethnocentrism by including two pieces that draw from the revolutionary reflections of Third World authors, Frantz Fanon and Paulo Freire.

The overwhelming emphasis is nevertheless on the U.S. political experience or, more generally, on problems derived from advanced industrial societies. There are no contributions by women, and with the exception of Ben Agger's essay, none of the contributors directly addressed questions emerging from the women's movement.

What was distinctive and singular about the critical theory of Adorno and Horkheimer and Marcuse was its repudiation of modernity and the promise of the enlightenment insofar as the scientific knowledge that made it possible to rationalize human existence also augured the "end of the individual." Objectified man in the totally administered society was the logical endpoint of the project of modernity.

Coupled with this pessimism, Adorno and Horkheimer (and, to some extent, Marcuse) mourned the passage of high culture and the promise of autonomy with the emergence of mass society and mass culture. It is these aspects of the older critical theory that allow us to view our narcissistic age and our illiberal liberation with a critical eye. Critical theory compels us to become conscious of the emptiness and the superficiality of life in the technocratic society where the falsely liberated, therapeutic personality has replaced the autonomous ideal.

Jurgen Habermas represents the second generation of critical theory, and his project has been to retrieve the essential optimism of the enlightenment and of critical Marxism while fully cognizant of the structural transformations in contemporary society that have made the rational and critical promise of these traditions problematic. In fact, one might say, that Habermas's work has been dedicated to the systematic reexamination of critical theory in order to reunite its powerful theoretical critique with a conception of action appropriate to advanced industrial societies.

The essays in this volume stand as a tribute to the theoretical richness of Habermas's writings. But, as one of the authors put it, discourse about critical theory tends to be very abstract and fails to make contact with those mainstream social scientists "in the trenches" who are still locked into a positivist epistemology for the lack of a better alternative (p. 253). What these essays intend is to demonstrate to colleagues in political science and sociology, in urban planning and in the policy sciences, that critical theory is a viable alternative to positivism.

For the most part the pieces succeed in moving the discussion away from the theoretical abstractions and towards practical questions. Frank Fischer demonstrates how a critical perspective on the Head Start program can challenge the claims of an empirical evaluation in the Westinghouse Learning Corporation's report. By exploring phenomenological questions about the subject's self-understanding of Head Start and critical questions about the ideal of society presupposed by the program, the narrow and distortive empirical measures of the Westinghouse study are placed in perspective. Similarly, John Forester demonstrates the value of critical theory to planners, whose roles are reinterpreted here from the perspective of Habermas's communicative competence. No longer viewed as simply technicians with expertise, planners emerge as agents in a discursive process aimed at democratizing the system that binds together the planner and his public.

Somewhat less innovative are the essays by Ray Kemp and Peter Grahame. Kemp argues that the Windscale Inquiry of 1977, which was to explore "a proposal by British Nuclear Fuels Ltd. (BNFL) to construct a thermal oxide reprocessing plant" (p. 177), turned out to be an inquiry serving the interests of "state and capital" and therefore an exemplification of the systematic distortion of the communication process. I doubt whether such a finding requires the analytical power of critical theory. Equally questionable is the essay by Peter Grahame, which seeks to use *Consumer Reports* as an ideal type of literature creating a focal point for critical opposition to the manipulative advertising of the major corporate producers. Things get carried a bit far when Habermas is invoked to conclude that *Consumer Reports* "occupies a unique position as a critical interlocuter of mass market definitions of satisfaction" (p. 149).

David C. Hallin writes informatively about the news media in the United States and claims that a technical and professional definition of their reportorial role prevents these media from providing the kind of active public sphere central to critical theory's project of democratic revitalization.

Along very different lines, Dieter Misgeld presents a powerfully convincing interpretation of Habermas's life-world colonization by using Paulo Freire's *Pedagogy of the Oppressed.* Essentially, Misgeld argues that linguistic colonization occurs when the concepts and definitions of the oppressors are employed by the oppressed to define themselves. The oppressed referred to here are not only colonized Third World peoples, but inhabitants of advanced industrial societies who have allowed their life-worlds to be transformed by the scientific knowledge possessed by the experts in the helping professions, the new agents of social control. John O'Neil similarly finds much complementarity between Habermas's emancipatory project and Frantz Fanon's and Paolo Freire's decolonization.

The two essays that launch the book, one by Ben Agger and the other by Timothy Luke and Stephen K. White, are disappointing. Luke and White straddle the fence on the question of the prospects for a more democratic society. On the one hand, they see the life-world colonization as enhanced by the informational revolution; on the other, they find "ambivalent potential" in the broadly defined "ecological path to modernity." Agger, while challenging "high-tech capitalism" as a panacea, points to the impending crises in the next stage of capitalist development. Despite its tacky metaphors of romance, affairs, and bedfellows, John Forester's second essay in this collection serves a useful purpose in juxtaposing the central tenets of policy analysis to the basic ideas of critical theory.

Alone among the contributors, Trent Schroyer boldly raises provocative questions about the adequacy of critical theory to understand the U.S. experience. Reminiscent of Tocqueville, and, more recently, of Robert Bellah, Schroyer argues that our civic and religious traditions, our "cultural surplus" give credence to claims of U.S. exceptionalism. It is the revolutionary and critical components of those traditions that Schroyer turns to as potential resources for the revitalization of the "universal principles for political participation" and a communicative ethics to ground a new consensus (p. 311).

There are ironies in a collection of essays on critical theory designed for a U.S. audience. The ideas suffer more than a sea change as one reads that *Consumer Reports* represents a critical negation of mass advertising, with no recognition that the consumer project is more rationally enhanced by the presence of such reports. There is irony also in the critical analysis of the Head Start program. While it reveals the broader horizons possible for the culturally deprived, little is made of the legitimation function served by Head Start, and there is no "critique" of the social reproduction of capitalism that takes place in the midst of this and similar programs. Perhaps Adorno was ultimately right when he argued that the only posture for critical theory is negation without resolution. For, in the post industrial age, it seems that a positive or practical application of critical theory may lead unintentionally to a new form of legitimation. The essays in this volume inadvertently pose that dilemma and deepen our interest in critical theory.

JAMES FARGANIS

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Between Philosophy and Politics: The Alienation of Political Theory. By John G. Gunnell (Amherst: University of Massachusetts Press, 1986. 240p. $25.00, cloth; $12.95, paper).

This trenchant work provides a "synthesis, clarification, and elaboration" of arguments made elsewhere by Gunnell alleging the "alienation" of academic political theory (p. ix). Not the first to suggest that dominant practices in this field are forms of intellectual masturbation, Gunnell nonetheless presses this central point with considerable zeal and with a delightfully acid tongue. He is thereby sure to provoke contention if not the "transformation" he desires.

Transforming the field requires an understanding of its present-day ills. Hence, the first chapter offers a reconstruction of the intellectual development (or rather devolution) of the field over the last three decades in order to identify a variety of historical forces that in turn help account for the underlying cause of the alienation of political theory today: its entrapment in and by philosophical and metatheoretical discourse, or "its absorption with metatheory" (p. 154), or (more creatively) its "philosophization" (pp. 28, 31).

This dreaded disease engenders in political theory a distorted and pretentious self-image. Symptoms include the following: the subject is ensnared in myths (identified on pp. 1 and 194); is unreflective about its parasitic relationship to "the transcendental and epistemological traditions in philosophy which . . . are themselves alienated enterprises" (p. 4); and is unable to achieve an authentic appreciation of its actual relationships to politics and political inquiry. Though fruitful relationships are virtually nonexistent, most theorists believe, or pretend to believe, that their estranged and rarified discourse provides authoritative standards, foundations, and illumination for both political practice and political inquiry, and on which, therefore, both are or ought to be dependent. They even believe, or make believe, that much of this discourse, being in some (distant) way about politics, is itself political.

These illusions and delusions serve a variety of purposes, but foremost among them is, of course, the bolstering of self-image and of the conviction that all this talk—about epistemology, methodology, explanation, and interpretation; about metaethics, normative foundations, and facts and values; about the universal dilemmas, fundamental concepts, and philosophical dimensions of politics; about historiography, hermeneutics, and the great tradition of epic theorists—actually constitutes an authoritative script for inquiry and an authoritative and authentic "theoretical and practical engagement of politics" (p. 138).

How might the patient be cured? Academic theory "redeemed"? Discourse "transformed"? Two prescriptions seem crucial. First, theorists must come to recognize and freely admit that political inquiry and politics are autonomous practices, quite capable of getting along without transcendental foundations and legitimations. Second, they must rethink "what theory is and can be in political theory." As we shall see, taking this cure can help theorists "come to grips with the question of the relationship of academic political theory to politics," though it cannot resolve this difficult problem (p. 155).

With respect to autonomy, Gunnell's position is the contextualist one that criteria defining practices and standards of good practice are internal to practice; consequently, second-order reflection on practice, no matter how well performed, cannot be automatically authoritative (though it *may* be influential). This position means that science does not need metascience and that politics does not need metapolitics. Like science, politics has its own standards and theories, which may or may not be influenced by academics. But that most of what passes for political philosophy and political theory today could possibly affect practice, Gunnell very much doubts. To suppose it could would be to suppose that it took political practice as its object and that it had an audience with which to interact. In fact, however, it takes philosophical objects, projects, and perplexities and *ascribes* them to politics. Thus, Rawls's work and the cottage industry it has inspired "is not about any human practice; it is not about any state of affairs. It is about concepts and logic." Similarly, MacIntyre assumes in *After Virtue* that moral relativism is a political rather than an academic problem, and that the latter is both the cause of the former "and the site of a solution" (pp. 176, 179).

What all this suggests is that political theorists should engage politics in the manner, for example, of that portion of the feminist literature that confronts an "existential problem . . . and that speaks to and for an actual audience" (p. 122). Such a prescription means only that *metatheory*, not *substantive* theory, be given up. Advancing *substantive*, *constitutive*, or *ontological* theory should be a primary task of academic political theory.

By *substantive* theory Gunnell means "that class of claims that establishes a domain of facility—what exists and the manner in which it exists—and provides the criteria of explanation, description, evaluation, and prescription" (p. 143). An example of such a theory, one on which Gunnell has been working for some years, is provided in the final chapter. It draws on theoretical (not metatheoretical) claims advanced in the philosophies of action and language, and thus postulates ontological claims about social phenomena that are familiar features of the literature on interpretive forms of social inquiry. It just happens to assert the conventionality of politics, thereby undercutting the contrary ontological positions of transcendentalists while underwriting an interpretive form of inquiry that would require taking conventional politics as its text.

Gunnell tries in this way to unite theory, inquiry, and practice. Because his (corrigible) theory entails the conventionality of politics, inquiry must proceed along interpretative lines. This in turn ensures that conventional politics will be the object of analysis, thus opening the *possibility* of practical engagement. By focusing on conventional political understandings and standards, the theorist just might find an audience interested in what he or she has to say. Of course efficacy is not guaranteed, but the alternative is hopeless. To continue the quest for transcendental political forms and standards is a sure loser even if such entities could be discovered. For these would resolve only transcendental crises and dilemmas and leave politics quite untouched.

It is this essentially political message that makes Gunnell's position a compelling and timely one. Unfortunately, debate will focus on the metatheoretical, methodological, and theoretical claims he advances; the irony of this situation will be examined in great detail; protests aimed at his exaggerations and stinging rhetoric will be heard from many quarters; and the thundering political silence will continue.

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Aspects of Toleration: Philosophical Studies. Edited by John Horton and Susan Mendus (London: Methuen, 1985. ii, 180p. $29.95).

John Stuart Mill's *On Liberty* has set the context for modern discussion of the principle of toleration. *Aspects of Toleration: Philosophical Studies*, edited by John Horton and Susan Mendus, is a collection of essays that attempt to put Mill in the context of the 300 years that culminated in his seminal essay, to examine and to clarify his philosophical assumptions, and to link his work to contemporary issues.

The essays in the collection are reworked papers presented at seminars and conferences supported by the Morell Trust based at the University of York, England. All but one of the authors is, or has been, a member of the political science or philosophy faculty at that institution.

Sexual, religious, and racial toleration are the basic themes of the essays. Racial toleration is discussed by Alex Callinicos. Religious toleration is the theme of David Edward's essay. Sexual toleration, in particular the toleration of pornography, is the subject of Susan Mendus's paper. These issues are dealt with collectively by Thomas Baldwin who uses them to examine the relationship between freedom and toleration. Peter Jones and John Horton, in separate papers, discuss the relationship between harm and toleration. Albert Weale and Peter Nicholson analyze the justification for, and the meaning of, toleration in modernity.

The essays are more interesting and more provocative of good conversation than the subjects might suggest. Horton makes us rethink Mill's belief that toleration is only limited by a test of whether the action being tolerated harms others. We do not always agree as to what is harmful: harm is not a neutral criterion. Jones argues that harm must be read to cover the moral effects of action: it may be licit to require seat belts because the consequences of not wearing one impose moral duties on others to aid the injured. Mendus raises pressing questions as to whether pornography might be corrupt in itself and thus beyond need to be judged by the harm principle.

Edwards and Weale base their case for toleration on the principle of respect for persons. Edwards argues that the most compelling case for protecting religions conviction against abuse, but not against criticism, should be based on this principle. Weale concludes that this principle provides the soundest foundation for toleration. Both sever Mill's argument from any lingering utilitarianism.

The best internal debate is between Baldwin and Callinicos. Baldwin argues that the duty to tolerate rests on others' right to freedom; toleration is obligatory in a free society. Callinicos, while rejecting Marcuse's concept of "repressive tolerance" argues that the requirements of freedom set limits on the practice of tolerance. Toleration should not be extended to fascist groups promoting violence.

Taken as a whole these essays might suggest the philosophical collapse of liberalism. Mill's assumptions, as analyzed, seem to provide an uncertain guide to modern society. Mill's philosophy seems to be judged shallow. Modern liberalism seems not to know how to defend itself against its enemies. Rampant pluralism tugs at old values: Can toleration extend to *Hustler* magazine, to virulent anti-Semitism, to the Moonies? The authors suggest that liberalism may be arrogant when rejecting questions of abortion, of the duty to protect crime victims, of the feminist critique of pornography.

But the authors do not draw this conclusion. They insist that the principles of liberalism, while needing to be rethought, are still to be trusted. The garden of toleration needs tending, not plowing under. Though these essays do not suggest a complete strategy for how toleration may be adjusted to pluralism, and do not suggest any final response to the threat of the Right or the Left, they do serve to continue a useful dialogue.

The philosophical analysis offered by this book should commend itself to U.S. audience. The United States is also an heir of Anglo-Saxon liberalism. The case studies, however, are almost excessively British. The U.S. reader is left to puzzle out the details of the *Gay News* case of 1977 that reaffirmed blasphemy as a crime. We are assumed to know all we need know about the National Front, a British right wing organization. Questions arising from AIDS, so crucial to a discussion of toleration on this side of the Atlantic, are not discussed. *Aspects of Toleration* should be read by U.S. citizens interested in the issues it raises; but it would be more useful to read it as a companion to Jacques Thirouk's third edition of *Ethics: Theory and Practice* (Macmillan, 1986). That work is also rich in analysis and the examples will be familiar to the U.S. reader.

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Political Cognition. Edited by Richard R. Lau and David O. Sears (Hillsdale, NJ: Lawrence Erlbaum Associates, 1986. x, 408p. $39.95).

For many years, political scientists have drawn from the work of social psychologists in efforts to understand the attitudes and behaviors of ordinary citizens. *Political Cognition* represents the latest merging of these two disciplines, and its focus reflects the cognitive revolution currently under way in social psychology.

The book is composed of papers presented at a mid-1984 conference among experts in political behavior and social cognition. The first of three major sections introduces readers to the social cognition perspective and illustrates ways in which this body of work might inform the study of politics. Composing the second section are eight papers describing empirical explorations of political cognition. These papers form the core of the volume. Essays written by political scientists synthesizing the empirical chapters and speculating about future prospects for studying politics from the social cognition perspective constitute the final section.

As the first section's chapters point out, social psychologists studying social cognition do not have grand integrated theories to offer political scientists. Rather, their work highlights concepts (e.g., schemas), empirical regularities (e.g., systematic errors in information processing), research methods (e.g., sentence completion tasks), and independent and dependent variables (e.g., reaction time) that suggest hypotheses and ways to test them. The empirical chapters here draw primarily on concepts developed in social cognition research while using the traditional methods and variables of political science.

Work in social cognition focuses largely upon memory: how information is selectively gathered from individuals' environments, stored in memory, accessed later, and integrated into decisions or judgments. A central tenet of social cognition work is that the structure of knowledge stored in memory has important impact upon how that knowledge is used and upon the judgments people make with it. Two people with identical knowledge about an object will sometimes make different judgments about it if the individuals differ in terms of the organizing structures, called schemas, linking their bits of knowledge together.

Expertise is the principal variable used in social cognition research to differentiate people with different knowledge structures from one another. People who are experts in a particular domain are thought to have more elaborately structured knowledge in that domain, allowing them to process information more quickly and accurately and to store larger volumes of relevant knowledge in memory. Experts are thought to be best prepared to infer unobserved characteristics of actors and events on the basis of observed ones and are thought to evidence substantial belief constraint.

The first five empirical chapters demonstrate that these general claims are readily applicable to political cognition. At first glance, these chapters seem to address a wide range of issues. One examines the bases of presidential candidate evaluations, another studies perceptions of candidates' issue positions, a third refines symbolic politics theory, and a fourth attempts to explain the decline in partisanship since the 1950s. However, all of these chapters compare citizens who are political experts (as gauged by political knowledge, exposure to political information, or interest in such information) to those who are political novices. This theme provides a useful frame with which to review these chapters.

Hamill and Lodge demonstrate that political experts are (1) especially knowledgeable about the ideological orientations of political actors and about the typical attitudes of liberals and conservatives, (2) quicker and more accurate when making ideology-related judgments, and (3) better able to remember ideology-related information. Lau shows that political experts are more likely to base their evaluations of political actors on considerations of group benefits, policy issues, and candidate personalities, therefore evidencing greater constraint among their political judgments. According to Conover and Feldman's evidence, people most interested in politics are likely to infer candidates' issue stands by using (1) the candidates' party affiliations in combination with perceptions of the parties and (2) the candidates' ideologies in combination with beliefs about liberals and conservatives. This is also evidence of stronger constraint among political beliefs. Sears, Huddy, and Schaffer show that among political experts constraint among values, issue positions, and ideology is greater, and Miller demonstrates that partisans (who might be thought of as party experts) (1) have more party-related information stored in memory, (2) are more likely to perceive differences between the parties in terms of policy stands and past performance, and (3) are more likely to evaluate the parties on the basis of the candidates who represent them, yet another form of constraint. All this and other evidence in these chapters builds confidence in social cognition's basic theoretical perspective and refines our understanding both of political cognition generally and of differences between political sophisticates and novices.

The critical reader is likely to find these conclusions quite reasonable but may nonetheless be a bit dissatisfied with these chapters. The schema concept appears prominently in all of them, but the authors present no direct evidence of the existence of schemas. Instead, their existence is either assumed (e.g., Sears, Huddy, and Schaffer) or is measured by (1) mentions of a concept in response to standard open-ended survey questions (e.g., Lau), (2) correct answers to quasifactual questions (e.g., Hamill and Lodge), or (3) willingness to place a political actor on a seven-point scale (e.g., Conover and Feldman). The goals of these chapters are to test predictions derived from ideas about schema functions; confirmations of these predictions are the only evidence here for the existence of schemas and for the accuracy of the theoretical perspective. However, since many of the hypotheses tested can be justified on theoretical grounds quite well without any mention of schemas, the skeptic may argue that the schema concept does not play as important a role in this research as the authors suggest.

The last three empirical chapters turn away from recent research in social cognition. Drawing on other work by social psychologists, the authors argue for the importance of three bases of political judgments that have received little attention in the past. Kinder argues that judgments of presidential candidates' personalities are potent bases for citizens' candidate preferences. He shows that assessments of competence, integrity, leadership, and empathy drive overall candidate evaluations over and above the impact of party, issues, and other such factors, and that the weights attached to personality assessments vary across candidates in intelligible ways. Tyler reviews literature documenting that citizens' judgments of procedural and distributive justice influence their evaluations of political actors and government more generally. Finally, Roseman, Abelson, and Ewing argue that people differ from one another in terms of the emotions they typically feel and that evaluations of political organizations depend in part upon the match between the emotions a citizen typically experiences and the emotions aroused by the organizations' appeals. In these chapters, social psychology is responsible for alerting researchers to these useful independent variables.

The empirical work in all these chapters can be criticized on a number of grounds. For example, Lau devotes no discussion to the fact that voters he claims do not have issue schemas nonetheless evidence strong effects of their issue attitudes on candidate evaluations (see p. 119). Conover and Feldman, Kinder, and Roseman, Abelson, and Ewing fail to provide empirically validated explanations for between-candidate, between-issue, and between-emotion differences they uncover. Sears, Huddy, and Schaffer's regression analyses testing indirect causal effects offer more support for their hypotheses than they recognize (see p. 179). Miller performs regressions with cross-sectional data in efforts to explain trends over time without performing the necessary longitudinal analyses. Tyler's regressions fail to deal with the problem of reciprocal causal effects. Hamill and Lodge document that political experts process political information differently than novices do, but they fail to link information-processing strategies to consequential political behaviors.

These problems and others are not trivial, but they do not discredit the important contributions of these chapters. Overall, this volume represents a top-notch integration of the newest ideas in social psychology with the sophisticated study of U.S. citizens' political thoughts. The implications of this body of work are far-reaching, as the final section's chapters illustrate, and it will be interesting and thought-provoking reading for both political scientists and psychologists.

JON A. KROSNICK

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Toward a Just Social Order. By Derek L. Phillips (Princeton: Princeton University Press, 1986. x, 460p. $50.00, cloth; $12.50, paper).

This ambitious book has three general aims. One is to give an account in terms of the best available sociological theory of how social order arises and is maintained. A second is to construct a rational justification from that and from the best available ethical and political philosophy for the kind of social order we *ought* to build and sustain: a *just* social order. And a third aim of the book is to argue for the legitimacy, within social science, of the sort of normative sociology represented by the conjunction of the first two aims. This last involves showing that normative propositions about justice, no less than explanatory ones about social order, are capable of rational justification.

Given Phillips's previous work, neither the aims of his new book nor its excellence comes as a surprise. His *Equality, Justice, and Rectification* (London: Academic Press, 1979) considers a few of the same normative issues, and both that book and his *Wittgenstein and Scientific Knowledge* (London: Macmillan, 1977) address the legitimacy of what is sometimes called normative sociology. This new book is characterized by masterfully lucid and compact exposition, together with accurate criticism, of some of the same themes. But its scope and its unrelenting emphasis on rationalistic ethical theory are startling. It deserves careful study, along at least the following lines.

First, the exposition and criticism. Wittgenstein once said that philosophy is the process of assembling reminders for particular purposes. Phillips follows that route over a vast terrain, summarizing solutions to the social order problem (private interest, situational, consensus, and conflict theories), recent work in ethics and political philosophy (Rawls, Nozick, Habermas, Gewirth), theories of socialization (Freud, Parsons, Kohlberg), major theories of law (with accounts of natural law theory, legal realism, positivism, and a long consideration of Ronald Dworkin's views), as well as recent accounts of children's rights and major approaches to the rational justification of an obligation to obey the law.

All of this material is deployed as part of the constructive argument of the book—meaning that the object is to weigh competing theories against each other and then to adopt, develop, and integrate the best of the lot into a coherent account of the just social order. This is a project fraught with the temptation to lay other people's theories out on Procrustes' bed, and the steadiness with which Phillips resists that temptation is admirable. As a result, the expositions he gives are almost invariably models of brevity, clarity, and accuracy (but two cautions are noted below). The expositions are intellectually rewarding to read quite apart from the general line of argument they serve.

The second area that deserves careful study concerns the suddenness with which Phillips occasionally incorporates other arguments into his own (or at least forms an alliance with them). In my view, he is too ready to use even his properly qualified version of Kohlberg's account of the form of moral development, and too ready to ignore both the methodological criticisms of Kohlberg's work and the lines of inquiry opened by Gilligan and others. This is not a mere quibble. To the extent that Kohlberg's critics can successfully attack the identification of moral development with the ability (and propensity) to think in terms of rationally justified, universalizable principles, Phillips's project in this book will be more difficult.

There may be a similar problem with Phillips's endorsement of Alan Gewirth's account, in *Reason and Morality* (Chicago: University of Chicago Press, 1978), of a rational foundation for the generic rights of rational agents to freedom and well-being. Those rights—and their rational justification—are central to Phillips's vision of the just social order. Gewirth's monumental book certainly deserves the central place Phillips gives it; it is the most rigorous and thoroughgoing rational justification of rights in existence.

Philosophers are likely to blink, however, at the firmness with which Phillips dismisses the most widely held and fundamental objection to Gewirth's deduction. Again, this is a point of some consequence for Phillips's argument. Gewirth is generally acknowledged to have shown that rational agents must, on pain of self-contradiction, assent to the proposition that certain sorts of freedom and well-being are necessary goods for all rational agents. What is at issue is the next move in his argument—the conclusion that rational agents must assent to the proposition that all have *rights* to freedom and well-being. The issue, here, put another way, is whether Gewirth has produced a foundation for a value-based (teleological) morality or, as he claims, a foundation for a duty-based (deontological) one. Phillips's project goes much more smoothly with the deontological foundation, and he is certainly aware of the controversy on this issue. It may deserve more attention than he devotes to it, however, given the central use he makes of Gewirth's arguments.

One more thing about Gewirth: one of the reasons Phillips finds Gewirth's work so congenial is that it is worked out in terms of the generic features of actual human agents—rather than in terms of the hypothetical, idealized rational agents so often used in ethical, political and economic theory. It is also important, philosophically, that Gewirth works from *within* an agent's point of view, coming to what he calls "dialectically necessary" conclusions. What he shows, strictly, is not that rights exist "out there" somewhere, but that an agent cannot without contradiction deny certain propositions. It would have been interesting to see what use Phillips could have made of this aspect of Gewirth's position.

The major area in which this book should repay close study, however, is in Phillips's outline of a just social order. The general features of the outline are familiar ones: effective and humane socialization; social control through a liberal democratic political order; an economic framework of market socialism. Nor are its more detailed features (children's rights in the socialization process, the importance of emotions, the justification for obedience to law) radically innovative. Phillips does not intend them to be. It is not his purpose to produce a *novel* vision of the just social order. Rather, what he wants to show is how a fairly familiar vision of it may be filled out and rationally grounded not only in what we know about the way things are but in what we know about the way things ought to be. The best thing about this book, in my view, is its sustained argument that we must put those two areas of knowledge back together.

LAWRENCE C. BECKER

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Philosophical Papers. Vol. 1, **Human Agency and Language.** By Charles Taylor (Cambridge: Cambridge University Press, 1985. vi, 294p. $39.50, cloth; $12.95, paper).

Philosophical Papers. Vol. 2, **Philosophy and the Human Sciences.** By Charles Taylor (Cambridge: Cambridge University Press, 1985. 340p. $39.50, cloth; $12.95, paper).

In these two volumes, the distinguished Canadian philosopher Charles Taylor has brought together 20 of his previously published papers, to which he has added two new essays and an illuminating introduction. Several of these essays—especially "Interpretation and the Sciences of Man" and "Neutrality in Political Science"—have attained near classical status since their publication (and subsequent republication in various edited volumes). Taylor's admirers and critics will, however, be disappointed to note that a number of his recent essays—for example, "The Philosophy of the Social Sciences," in *Political Theory and Political Education*, edited by Melvin Richter (Princeton, 1980) and "Understanding in Human Sciences" in *Review of Metaphysics* 34 (1980)—are unfortunately absent from the present collection. Even with those regrettable omissions, however, these two volumes are remarkable for their range of learning, subtlety of argument, and philosophical acumen.

Despite the appearance of diversity—ranging from "Hegel's Philosophy of Mind," "Cognitive Psychology," and "Theories of Meaning" in volume 1 to "Understanding and Ethnocentricity," "Foucault on Freedom and Truth," and "What's Wrong with Negative Liberty" in volume 2—these essays are, as Taylor confesses, "the work of a monomaniac" armed with "a tightly knit agenda" (1: 1, 8). A number of themes recur. Foremost among these is Taylor's attempt to articulate and justify an alternative account of human reason, action, and agency. He is deeply dissatisfied with the "modern" (i.e., post-seventeenth century) view that the "values" we hold represent individual choices from among a range of options, any "mix" of which is equally plausible and none of which is intrinsically preferable to any other. Against this modern orthodoxy Taylor argues the case for a radically different account of who and what we human beings are. Our identities are not contingently or causally related to the values we hold and the choices we make, but are constituted by the ways in which we understand ourselves and interpret the meaning of our own and others' actions. Like Heidegger and Gadamer (and, more distantly, Herder and Humboldt) Taylor holds that the human world is an "expressive" or communicatively constituted world of utterances, gestures, and actions whose meaning must, in order to be intelligible and justifiable, be interpreted by the participants themselves. Unlike other creatures, we human beings are by nature "self-interpreting animals" (vol. 1, chap. 2). These interpretations are constructed out of the concepts and categories of understanding made available to us by the language we speak.

Yet the moral language of modern man, Taylor argues, has been deeply imbued with and impoverished by naturalistic theories of language, meaning, and action (vol. 1, chaps. 1-4, 9-10). More than a metatheory or a philosophy of social inquiry, naturalism constitutes a system of meaning and "deep evaluation" in terms of which modern men and women have attempted—disastrously, Taylor thinks—to order their lives and their communities. Several of these essays are, accordingly, devoted to criticizing naturalism in its several guises and varieties, including behaviorally oriented political science (vol. 2, chaps. 1-3).

Taylor is scarcely less critical of the "deconstructive" turn taken by "post-structuralists" like Derrida and Foucault (vol. 2, chap. 6), and their greater grandfather Nietzsche. Their theories of human language, agency, and action are "underdemonstrated," "impressionalistically [*sic*] argued for," and "hostile and dismissive towards the scientific outlook" (1:7). What is needed, Taylor maintains, is not more derision but a better and more critical understanding of the sources and attractions of naturalism. The elements of such an interpretation are tantalizingly sketched in his lucid analyses of "political atomism" and the liberal-individualist notion of negative liberty (vol. 2, chaps. 7 and 8) and, more particularly, in "The Concept of a Person" (vol. 1, chap. 4).

Taylor is the first to admit that these essays are exploratory rather than definitive. These "promissory notes" will, he says, soon be redeemed in a larger and more systematic work. Until that arrives we are very fortunate indeed to have in hand this weighty and impressive preface.

TERENCE BALL

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AMERICAN POLITICS

Intergovernmental Management: Human Services Problem-Solving in Six Metropolitan Areas. By Robert J. Agranoff with the assistance of Valerie Lindsay Rinkle (Albany: State University of New York Press, 1986. x, 199p. $39.50, cloth; $12.95, paper).

Robert Agranoff's concisely written monograph springs from the basic authors and texts toward which most political scientists nod recognition when embarking on an exploration of intergovernmental relations (IGR). With appropriate homage paid to David Walker, Deil Wright, and the Advisory Committee on Intergovernmental Relations (ACIR), Agranoff moves toward understanding the implementation of policies that derive from an intergovernmental context.

Through a series of fairly logical steps, the author explains that intergovernmental management (IGM) involves daily transactional arrangements among public administrators and among their organizations. His focus is on local government and the quasigovernmental and not-for-profit agencies that constitute various policy arenas. Such a concentration on local agencies and their interrelationships is justified by arguing that the local level is where federal and state programs ultimately have to work.

To provide an action orientation to IGM, Agranoff chooses problem solving as a model for understanding interorganizational coordination. Although he introduces the concept as a rational, formal, systematic process, he concludes from the study that it is not always rational, formal, or systematic, nor does it always explain the multiple relationships in a complex intergovernmental environment. It is useful, however, for understanding that successful implementation of human service programs involves the use of creative and conflict management as techniques for coordinating agencies that must work together.

The research to which these various ideas are applied consists of six exhaustive case studies of IGM. Drawing on field work of over 80 persons in six cities, Agranoff analyzes case studies of human services implementation in a common conceptual framework. By painstakingly adhering to a rigorous methodological sequence, he has largely escaped the idiosyncratic liabilities of the case study. Hence, his conclusions can be defended on grounds of intuitive logic as well as on its well-documented comparability. No doubt only the most ardent students or practitioners will wade through the densely detailed cases; however, they are readable and may serve as useful lessons for the intergovernmental or public administration classroom.

Of most lasting usefulness, perhaps, are the general findings about IGM and the research agenda Agranoff proposes in the last chapter. Since he places his objectives in understanding the management of interorganizational programs within a highly fragmented IGR system, rather than proposing reform of that system, Agranoff's findings are most relevant to the public administrator or to those who teach such practitioners. In a larger sense, his rich research agenda speaks to a wider range of serious IGM students who may be curious about "getting things done" in a day-to-day, operational fashion.

Given the increased demands of current devolution and decreased funding from Washington, such research is critical not just to creative management but to agency survival as well.

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Making the Managerial Presidency: Comprehensive Reorganization Planning, 1905–1980. By Peri E. Arnold (Princeton: Princeton University Press, 1986. xiv, 374p. $37.50).

Peri E. Arnold has made an important contribution to the literature on the presidency. His analysis of comprehensive reorganization planning from Theodore Roosevelt's Keep Commission to Jimmy Carter's Reorganization Project identifies the critical role played by reorganization planners in developing and gaining acceptance for the concept of the managerial presidency. While reorganization rhetoric has stressed the neutral goals of economy and efficiency, the central purpose of reorganization has been to enhance the president's capacity to manage and direct the executive branch. Arnold concludes that "at one and the same time reorganization planning aimed at strengthening the presidency while presenting the issue of enhanced presidential capacity as merely managerial and irrelevant to politics" (p. 159).

To some extent Arnold covers the same ground as previous studies of federal reorganization and reorganization projects by Oscar Kraines, Richard Polenberg, Herbert Emmerich, William Pemberton, Harvey Mansfield, Sr., and Ronald C. Moe, but he has unearthed a good deal of fresh material. He provides the first detailed examination and evaluation of the studies done by the Joint Committee for Reorganization during the Harding administration, President Eisenhower's Advisory Committee on Government Organization, President Johnson's Price and Heineman task forces, and President Nixon's Ash Council. He demonstrates that each of the reorganization efforts, while established under different political auspices and ostensibly for different purposes, shared common values and operated from a common set of theoretical assumptions.

The continuities are striking. Arnold correctly observes that the Brownlow Committee, rather than being an innovator, distilled "a tradition of thinking in public administration that reached back to the work of the President's Commission during the Taft administration" (p. 116). Scholars have largely ignored the Harding administration's Joint Committee for Reorganization, but it anticipated the Brownlow report with its emphasis on the managerial centrality of the presidency in the executive branch and its concerns with budgeting, departmental organization, and presidential staff resources.

By focusing almost exclusively on reorganization commissions, committees and task forces, Arnold underestimates the role played by the Bureau of the Budget in the 1940s and 1950s in articulating and promoting the doctrines of executive supremacy in administration and the managerial presidency. While presidents may have appeared to rely primarily on ad hoc reorganization vehicles, this was not, in fact, the case. Arnold does note the close relationship between the Bureau of the Budget (BoB) and the first Hoover Commission and BoB's influence on the commission's report on management of the executive branch. The bureau also provided the staff support for Eisenhower's Advisory Commission and Johnson's Price task force on reorganization. The bureau was responsible for developing a reorganization program and implementing the provisions of the reorganization act and it was by no means subordinate to the ad hoc groups.

While stressing the importance of the attempts by reorganizers to transform the president into a manager, Arnold concludes that the effort has failed. He believes that the managerial presidency may become "a trap, offering increased capacity and influence to presidents but creating even greater expectations about presidential performance" (p. 362). Many now question whether the doctrines advanced by the Brownlow Committee and the Hoover commissions are relevant for the federal government as it is organized and operates in the 1980s. It is significant that President Reagan has criticized prior reorganization efforts as being concerned too much with structure and too little with process. Control of procedures and regulations becomes more important than structure when the federal government increasingly relies on nonfederal agents to administer its programs.

Making the Managerial Presidency is likely to be the definitive work on federal reorganization. It has the virtue of being readable—something that cannot be said of many books on government management and reorganization. Despite its price, it merits a wide audience.

HAROLD SEIDMAN

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Apathy in America, 1960–1984: Causes and Consequences of Citizen Political Indifference. By Stephen Earl Bennett (Ardsley-on-Hudson, NY: Transnational, 1986. x, 198p. $30.00).

Are U.S. citizens more apathetic about politics than they used to be? If so, what difference does it make? These are the questions that guide Stephen Earl Bennett's analysis. Bennett makes the concept operational with University of Michigan survey data by combining a question on interest in political campaigns with a question on interest in politics in general.

Beginning with a cursory glance at U.S. history, Bennett reviews the trends in voting behavior and various theories of political participation. Next comes one of the highlights of his book, a careful justification of the method described above, which he calls the "political apathy index." Unlike other conceivable scales, it measures only the degree of political *interest*, and not behavior, partisanship, or other concepts that have muddied other indexes. Always admirably aware of just how far his data can take us, he finds the index fairly stable from 1960 to 1984, although somewhat lower in recent years than in the late 1960s and early 1970s. It correlates with the variables familiar to those versed in the literature of political behavior: education, age, gender, degree of partisanship, sense of civic duty, and external efficacy. Bennett also seems taken with generational explanations and argues them well. Those born in the 1940s were especially interested in politics in the 1960s, and the elderly are increasingly interested in recent years. The apathetic, as expected, do not follow politics in the mass media, know much about public affairs, vote, or engage in other political acts.

Bennett has produced a well-argued and diligent analysis of a phenomenon important in democratic theory. Some of the data should have been presented in a manner easier for the reader to grasp, such as more simplified tables and some graphs, and the author's gingerly use of multiple regression should have been augmented by some path analysis to flesh out the causal sequences. There are a few methodological glitches and points where the data presented do not bear out his conclusions. The author's own admonition about not confusing interest with voting turnout is ignored in the historical overview, and the discussion of generations should have been informed by that literature. These are peripheral qualms about an otherwise admirable job of data analysis and presentation.

Ultimately a study such as this raises some of the most fundamental questions of the relationship between citizens and the state. As proficient as he is at survey analysis, and although he does summarize the writings of various theorists and scholars about the proper degree of political behavior, Bennett seems relatively uncomfortable with macro-level analysis. Noting in several places how little political participation U.S. citizens engage in, he does not begin to explore why this is so. No attempt at comparative analysis is made in order to ascertain if U.S. citizens are unusual in this regard, and, if so, why. Theorists of a variety of ideological backgrounds have attempted to relate macropolitical phenomena to declining citizen involvement, from Samuel Huntington and his "democratic distemper" to Michael Parenti's rejection of democratic capitalist politics. Bennett mentions Parenti, but only to dismiss his position as "tendentious" (p. 32). But survey data, being at the micro level, cannot give us a picture broad enough to get at these essential questions.

These limitations, one must add in Bennett's defense, are common among those who engage in survey research. He has produced a fine piece of micro-level analysis of an important subject. Perhaps he will next turn his talents to a system-level explanation of why many people just don't care.

HOWARD L. REITER

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Politics and Social Welfare Policy in the United States. By Robert X. Browning (Knoxville, TN: University of Tennessee Press, 1986. xiv, 205p. $18.95).

American Social Welfare Policy: Dynamics of Formulation and Change. By David A. Rochefort (Boulder, CO: Westview Press, 1986. xii, 206p. $22.00, paper).

Since the late 1970s, an expanding empirical literature on the growth of the welfare state in the affluent market-oriented democracies has significantly altered interpretations of welfare policy determination imparted by the industrial society theorists of previous decades. Robert Browning begins his new book on the post-World War II growth of the U.S. welfare state by echoing an important theme of this literature—that ideology and political institutions matter.

Browning initially hypothesizes that the partisan balance in Congress, the party of the president, the interaction of these factors, the electoral cycle, economic conditions, shifts in the jurisdiction of relevant congressional committees, and incrementalism determine variations in social welfare expenditures. The bulk of the analysis actually focuses on the hypothesis that Democratic presidents and Democratic congresses, particularly those with large numbers of nonsouthern House Democrats, facilitate welfare spending growth. Consistent with these propositions, Browning shows that Democratic presidents are four times as likely as Republican ones to initiate new welfare programs and that Democratic congresses actively initiate new programs under both Democratic and Republican presidents. However, an examination of real percentage growth rates reveals that total welfare spending and income security outlays (by far the largest welfare category) grew most rapidly under three Republican presidents (Eisenhower, Nixon, and Ford) and one Democratic president (Johnson). The Reagan administration is the one notable exception to the tendency for this spending to grow more rapidly under Republican presidents. Growth of in-kind spending, such as outlays for health, education, and social services was most pronounced during three Democratic administrations (Truman, Kennedy-Johnson, and Johnson), although spending accelerated significantly during the second Eisenhower and Nixon presidencies.

Regression analyses of the welfare spending effects of Democratic presidents and the number of nonsouthern House Democrats, controlling for unemployment rates, confirm that Democratic presidents and the number of nonsouthern House Democrats did not facilitate growth in total or cash welfare outlays. Statistical models actually reveal that the number of nonsouthern House Democrats depressed the growth of these expenditures during both Democratic and Republican presidencies. The most notable finding to emerge from the analyses is that in-kind welfare expenditures grew more rapidly as the number of nonsouthern House Democrats increased during a Democratic presidency. Yet Browning is able to obtain this result only if Jimmy Carter, a Democratic president faced with significant pressure to control in-kind outlays, is treated as a Republican when coding presidential administrations.

Browning offers several plausible explanations for these findings. He argues that the generally higher rates of spending growth during Republican administrations were the results of Democratic congresses "bidding up" the social spending requests of Republican presidents. This argument may indeed explain some of the observed patterns in the data and statistical findings. However, the explanation is partially undercut by the statistical analyses clearly showing that increases in the number of nonsouthern House Democrats, which presumably make Congress more prowelfare, systematically depressed total and cash welfare spending growth during Republican administrations. Browning interprets this finding as a product of the relationship between recessions and Democratic electoral success. In his own words, "Their increased numbers in Congress are usually associated with declining expenditures for social programs associated with declining unemployment" (p. 130). However, this explanation holds only if one does not account for the welfare-spending effects of unemployment; Browning's finding that nonsouthern House Democrats dampen the growth of total and cash welfare spending emerges from statistical models that control for unemployment.

The most plausible explanation for these anomalous findings is that Browning fails to account systematically for many likely sources of variation in social welfare outlays, attempting instead to attribute much of the variation in the data to partisan control of Congress and the presidency alone. For instance, Browning largely ignores such widely discussed determinants of social welfare spending as changes in the sizes of the poor and elderly populations, economic growth rates, extra-institutional collective action by black Americans, and structural changes within the economy, among others. Although the book provides a wealth of aggregated and disaggregated data on welfare programs and supplies several useful insights into the policy-making roles of Congress and the president, failure to account systematically for these and other forces simply impairs our ability to disentangle the causal roles of partisan and nonpartisan factors.

In several ways, David Rochefort's new book on the sources of social welfare policy is more satisfying. Rochefort's thesis, as he admits, is not entirely original. James Patterson's *America's Struggle against Poverty, 1900–1980* (Cambridge: Harvard University Press, 1981) is excellent companion reading. The author's basic argument is that major innovations in social welfare policy are the results of changing images of individual responsibility and perceptions of the seriousness of the social problem and its causes held by policy makers, activists, and the general public. Images and definitions of social problems change as the result of a complex interplay among historical events, scientific discoveries, social research, shifts in professional and disciplinary paradigms, demographic changes, and broad intellectual and political currents. In other words, mass and elite attitudes, social scientific ideas and theories, and concomitant problem definition determine policy. This social images perspective on social welfare policy formation is developed in the context of a critique of existing theoretical perspectives. The plausibility of the social images perspective is arguably established by case studies of the enactment of the Community Mental Health Centers Act of 1963; of passage of important policies directed at the elderly including the Older Americans' Act, Supplemental Security Income, Medicare, and amendments to the Social Security Act between 1965 and 1972; and of Richard

Nixon's failed Family Assistance Plan.

Rochefort's thesis, however, is not without problems. The principal theoretical deficiency is the author's lack of sensitivity to how ideas of policy makers may interact with self-interest and ideology and to how action based on ideas may be conditioned by policy maker reaction to the conscious mobilization of group interest. While Rochefort does not discount such factors completely, he explicitly distinguishes the social images perspective from the view that economic and other material interests are central to an understanding of policy formation (p. 147). However, numerous examples of the policy-making roles of these widely discussed factors may be gleaned from Rochefort's own case studies. Sensitivity to the interplay of ideas, interests, and ideology during theory development would have improved the theory's richness and applicability.

In sum, both books, despite the aforementioned problems, make a contribution to the literatures on social welfare policy formation and policy studies more generally. They also point to a deficiency in the rather large literature on U.S. welfare policy; we have numerous partial accounts of welfare policy determination and few efforts at theoretical integration and subsequent empirical analysis.

DUANE SWANK

Marquette University

Cloning and the Constitution: An Inquiry into Governmental Policymaking and Genetic Experimentation. By Ira H. Carmen (Madison, WI: University of Wisconsin Press, 1985. xv, 223p. $22.50).

Two logics permeate modern science. As a *form of knowledge*, scientific inquiry is defended as an intrinsic good, obtained in a forum of free inquiry, underwritten by the presumption that only the most compelling (and rare) circumstances should limit the right to pursue knowledge wherever it may lead. This logic leads to the ideology of basic science. As a *form of power*, the scientific enterprise manifests itself in the Baconian formulation of science as a form of control and conquest and an instrument for human betterment. Seen this way, science becomes the servant of the state, the corporation, or of whoever is able to command it. This leads to the ideology of science as a national resource, to be harnessed, channeled, and ultimately controlled for policy-determined objectives.

Genetic research since the Watson and Crick breakthrough—specifically recombinant DNA, gene splicing, and cloning—confront scientific researchers and policy makers with the acute issue of the social control of a new application of fundamental knowledge that has the potential for both very useful and very harmful consequences. It is to the political and particularly the constitutional/legal aspects of genetic experimentation that this book addresses itself. Carmen's introductory chapter, "Science in American Constitutional History," is densely packed with information and provocative generalizations, and provides a succinct overview of the history of science in the U.S. polity. While not comprehensive, it is a stimulating discussion of the constitutional context of U.S. science policy. Chapter 2, "Cloning as American Constitutional Freedom," argues that DNA research, as a specific form of scientific activity, is entitled to a considerable measure of constitutional protection. He maintains that "just as government lacks carte blanche to manipulate free expression through the purse strings, so it lacks carte blanche to manipulate scientific experimentation in general and recombinant experimentation in particular by attaching capricious constraint to munificent reward" (p. 35). Chapter 3, "The Recombinant DNA Debate as Constitutional Debate," describes in considerable detail the contours of that debate, with all the major actors making their appearance on the constitutional stage, playing their parts, and making their case. Carmen's prism is the constitutional issues raised by the controversy, the manner in which scientists, National Institute of Health (NIH) officials, and others reacted to a social phenomenon "prompted by a unique methodology and potentially unique outcomes" (p. 60). He concludes that scientific development in this area (as, no doubt, in others!) is "capable of causing not only culture shock but constitutional shock" (p. 61). His focus is on exploring the dynamics and consequences of developments in a volatile area of scientific experimentation in terms of the impact on the political domain. The complexities and subtleties of that process and of the issues brought to the fore are carefully and neatly delineated.

Carmen undertook a series of interviews with a random sample of recombinant DNA scientists and biosafety committee chairmen at universities (the sample is rather small). Chapter 4, "Cloners and Their Watchdogs," is based on these questionnaire and interview materials. The data indicate that the researchers are "as a whole much more receptive than are the Institutional Biosafety Committee (IBC) chairmen to the NIH model of review and oversight through binding norms of laboratory decorum" (p. 135). Not surprisingly, both researchers and chairmen (who are typically scientists themselves) overwhelmingly support the proposition that scientific inquiry is a form of constitutionally protected free speech. Scientists believe that governments are "limited in their capacity to impose strings" upon their work (p. 151). This may be wishful thinking to some degree, since the partnership of science and government, is often itself a subtle form of control in the interest of those in power.

The concluding chapter, "Recombining Constitutional Issues," is woven around Carmen's concept of "a living constitution" in which the new challenges and demands of change and accommodation develop into a "new constitutional profile . . . to meet the many challenges of genetic engineering, while at the same time keeping faith with yet deeper patterns of right conduct" (p. 161). Carmen sees politics as an essentially neutral activity. "Like cloning itself, politics can be a tool for achieving any number of purposes . . . which, very often . . . are not so neutral in their social implications" (p. 170). The neutrality of the process is open to question, since it ignores the issue of the infusion of ideology, the shaping of knowledge by the social forces at a given historic period. Implicit in Carmen's model is a refined version of the invisible hand, a muted hymn to the virtues of the pluralistic model of U.S. politics. For a less sanguine view, one which also focuses on institutional parameters and control of U.S. science, the reader might turn to David Dickson's *The New Politics of Science* (New York: Pantheon, 1984).

Notwithstanding these caveats, Carmen's argument is tightly constructed and impressive in its command of the constitutional aspects as these relate to the history of the scientific and political debate over recombinant DNA research. Carmen provides much incisive scholarship on the profound problems with which he grapples. For the serious student and scholar a close and careful reading should prove most rewarding.

JOSEPH HABERER

Purdue University

Contemporary Constitutional Lawmaking: The Supreme Court and the Art of Politics. By Lief Carter (New York: Pergamon Press, 1985. xviii, 217p. $29.50, cloth; $12.95, paper).

This book's purpose is to get jurisprudence out of a bind that, Carter says, flows from two facts: first, modern philosophy (pragmatism, hermeneutics, and deconstructionism) tells us that belief in the determinateness of legal texts, the intelligibility of the intent or goals of lawgivers, and the attainability of a national consensus about fundamental political values are naive pipedreams; but, second, we also know that community can only be maintained if citizens believe in the normative goodness of the community and their participation in it. Professor Carter offers an "aesthetic theory of jurisprudence" as the way out of this bind.

Early sections of the book describe U.S. constitutional history as a series of discrete events that cannot be understood in the light of any coherent legal principles, although there are some minimal political patterns observable in it (e.g., judicial decision making based on the justices' hunches about social conditions and on their normative values). Interpretivism (which he calls "preservatism") is sharply dismissed on the basis of empirical observations (judges have not acted that way) and theory (hermeneutics shows that interpretation is really a form of creation). Professor Carter takes an interesting trot through recent constitutional scholarship (Wechsler, Bickel, Choper, Ely, Horowitz, Chayes, Miller, Perry, and the critical legal studies movement), finding various attempts to provide political norms of goodness for evaluating constitutional decisions to be deficient, especially because of their obsession with the false question of legitimacy, which is a surrogate for fundamental and scientifically unanswerable normative questions of political philosophy. He then discusses "normative alternatives to interpretivism" (e.g., pragmatists, Posner, Rawls, Dworkin, Murphy, Fiss), which are inadequate but do, more or less, contribute elements to the "aesthetic theory of jurisprudence" developed in the last two chapters.

According to this aesthetic theory, there are no right answers in constitutional law on the basis of legal reasoning. Yet we cannot stop there, because we need genuine faith in the existence of moral principles in order to make community possible. What makes for "good" opinions is good performances, which have the power to constitute community by creating momentary visions of coherent normative order, thereby sustaining faith in our ability to converse morally. If there is any substantive value implied in this approach it is the need to respect and protect individual dignity, without which conversations about political goodness cannot proceed. We need those conversations to create the experience of order and meaning so as to "protect us from the dark"; that is, the fact that, although meaning is essential to the survival of the race, it is an illusion. The "truth" of a constitutional opinion, accordingly, lies in the fit it persuasively creates between the facts of the case, the rules that the Court's audiences deem to bear on the dispute, beliefs about the empirical nature of the community, and normative claims that exist in common discourse.

Professor Carter's theory is an illuminating display of what happens to constitutional law when it becomes rooted in modern philosophical assumptions that destroy the possibility of genuine philosophy or, indeed, of any meaningful speech. If we're all just huddling in the dark and all meaning is illusion—although it is not clear how we can know the truth that we are incapable of knowing the truth—conversations are no better than silence. It is doubtful, moreover, whether his theory really constrains justices, giving us evaluative criteria for distinguishing between better and worse opinions. The assumption in favor of human dignity seems gratuitous, and in its absence the community could respond favorably to performances that create "visions of coherent order" that look like hell. If all meaning is illusion—and Professor Carter explicitly "rejects at the outset any framework based on statements of the universal rights of man" (p. 161)—there are no grounds for restricting individual freedom, but no grounds for defending it either.

Noninterpretivists will find this book an imaginative, even daring, effort to elaborate a new justification for an activist judiciary. Interpretivists—those who believe that judicial review should be confined to enforcing the clear command of the Constitution—will, in their own way, find it a useful example of what happens when the very possibility of interpretation is rejected.

CHRISTOPHER WOLFE

Marquette University

No State Shall Abridge: The Fourteenth Amendment and the Bill of Rights. By Michael Kent Curtis. (Durham, NC: Duke University Press, 1986. xii, 275p. $24.95).

Some authors have all the luck. Michael Kent Curtis's good fortune is to have his book on the meaning and intentions of the Fourteenth Amendment published at a time when the legal community can still be heard buzzing over the remarks of the attorney general to the effect that the Bill of Rights does not apply to the states, that the familiar theory of incorporation is a constitutionally suspect judicial innovation. The book, therefore, possesses a timeliness that no doubt will earn for it careful scrutiny from those with the potential for influencing our constitutional law.

Curtis, of course, is not entering virgin territory. Inquiry into the intentions of the framers of the Fourteenth Amendment, specifically whether or not these men expected their revised Constitution to nationalize the protection of civil liberties, has long been the subject of intense scholarly and judicial interest. In arguing that the rights in the Bill of Rights were rights that, through the first section of the amendment, limited the powers of states as well as the federal government, Curtis aligns himself with Crosskey and Justice Black in opposition to Fairman, Berger, and most of the justices who have sat on the Supreme Court since 1868. He suggests that the failure to comprehend the underlying purpose of section 1 accurately is attributable to a neglect of the broader historical context from which the Fourteenth Amendment emerged. The only way to make sense of the issue of intentionality is to examine the efforts of the framers against the backdrop of the antislavery crusade that set the political agenda in the decades leading up to constitutional change.

Curtis focuses his attention upon the constitutional and political thought of the Republican party. He summons considerable evidence to demonstrate that Republicans believed that the Bill of Rights limited or should be made to limit the powers of the states and that this was not an issue that divided conservatives and radicals. There had long been dissatisfaction with the South's interference with rights guaranteed by the Bill of Rights (notably the right of free speech). The determination to remedy this grievous anomaly, according to Curtis, was the motive force behind the sponsors of the first section. In developing his thesis that the privileges and immunities clause was to be the principal vehicle through which this crucial task was to be accomplished, the author provides a detailed, careful rejoinder to the contrary interpretations of scholars such as Fairman and Berger, whose work has had considerable influence in shaping the course of constitutional development.

Curtis is careful not to overstate his conclusions. "In a real sense one can never prove that the amendment was designed to apply the Bill of Rights to the states" (p. 217). It is impossible to read this book without taking seriously the strong possibility that this was precisely the design of those who crafted the amendment. The debate will continue, and Curtis's significant volume should assume a prominent place in the ensuing discussion. That discussion should seek to resolve two related issues of fundamental importance that are not directly addressed in *No State Shall Abridge.*

The first is suggested by an increasingly observable phenomenon: the tendency of scholars seeking a broader role for the Court in guaranteeing rights to cite as authoritative the work of Fairman and Berger as a necessary step in the abandonment of original intent. Curtis, who more than once makes evident his endorsement of such an expansive role, must (as Justice Black did) confront the possibility that his interpretation of the Fourteenth Amendment will in the end be used to limit the Court's role. Thus, his findings suggest that the privileges and immunities clause protects the fundamental rights of U.S. citizens against state intrusion, but only those rights stipulated in the document. To this reviewer, at least, he has not produced evidence that would support the contention of John Hart Ely that this clause was a delegation to future constitutional decision makers to protect rights nowhere specifically indicated in the document. the radical implications of the Republican achievement may not appear so radical within a transformed jurisprudential environment of noninterpretive review.

Perhaps inadvertently, then, Curtis's work will be as much of a challenge to activists as to self-restrainers. This suggests one final point. From Chief Justice Marshall's opinion in *Barron* v. *Baltimore* in 1833, to *Dred Scott* v. *Sandford,* to the post-Civil War amendments decisions, and down to our own time, the Supreme Court, according to Curtis, has been consistently wrong in its understanding of the responsibility of the states for the protection of individual liberties. If true, then, it is more than just a rhetorical ploy to wonder about increasing the authority of an institution that has made such a mess of our constitutional liberties over so many years. Curtis's impressive book should find a receptive audience on both sides of the debate over the appropriate exercise of judicial review.

GARY J. JACOBSOHN

Williams College

Minority Vote Dilution. Edited by Chandler Davidson (Washington, DC: Howard University Press, 1984. xi, 298p. $24.95).

The Joint Center of Political Studies, which commissioned this volume, is a research institute that conducts public policy studies of "special concern to black Americans" and promotes "informed and effective involvement of blacks in the political process" (jacket copy). The underlying assumption of this book is that minority vote dilution is "the most common form of electoral discrimination in the United States today" (p. vii). In 12 chapters, 16 authors define minority vote dilution, analyze its history, parse statutes and judicial decisions, and recommend legal doctrines, electoral arrangements, and enforcement mechanisms to root out minority vote dilution.

These essays are a powerful example of the use of social science to advocate public policy. Although the policy goals were plainly foreordained, that has not compromised the high quality of this book's scholarship.

Indeed, this volume's assault on multimember districts, the leading contemporary method of minority vote dilution, has already borne fruit. The Supreme Court, in *Thornberg* v. *Gingles* (54 U.S. Law Week 4877 [1986]), adopted an interpretation of the Voting Rights Act that permits "results-based" challenges to multimember legislative districts. The justices cited four chapters of this book and other works of several of its authors as support for their decision.

Minority vote dilution is broadly defined by Chandler Davidson as "a process whereby election laws or practices, either singly or in concert, combine with systematic bloc voting among an identifiable group to diminish the voting strength of at least one other group" (p. 4). At-large elections, racial gerrymandering, anti-single shot ballot rules, slating groups operating in either primary or general elections, decreased size of governmental bodies, and runoff requirements have all been used to dilute minority representation. This volume devotes a chapter to each of the first four of these vote dilution devices. J. Morgan Kousser, however, goes beyond this short list of dilution techniques: in deriving lessons for a "Second Reconstruction" in the 1970s and 1980s, he identifies 16 methods used to hamper black political power during the First (post-Civil War) Reconstruction.

Several authors strongly criticize the Supreme Court for insisting in *City of Mobile* v. *Bolden* (446 U.S. 55 [1980]) that multimember districts unconstitutionally dilute minority voting only where the intent or purpose is discriminatory. Peyton McCrary, J. Morgan Kousser, and Chandler Davidson and George Korbel establish in separate chapters that southern progressive voting reforms—including at-large elections, multimember districts, and commission government—had a discriminatory purpose or intent. Armand Derfner decisively demonstrates that the Voting Rights Act amendments of 1982 were designed to overturn *City of Mobile* v. *Bolden*'s purpose or intent requirement. *Thornburg* v. *Gingles* adopts that position.

James Blacksher and Larry Menefee go still farther: they reject established constitutional doctrine that discriminatory intent or purpose is essential to establish a Fifteenth Amendment voting rights claim. The Supreme Court not only relied on Fifteenth Amendment cases to establish the one-person-one-vote doctrine under the Fourteenth Amendment, but it detected violations of that doctrine entirely on the basis of "effects" (i.e., numerical variations) without any attention to intent or purpose. An effects test to establish Fifteenth Amendment violations of black voting rights, through various dilution devices, also seems warranted.

An insightful analysis of administrative enforcement of section 5's preclearance of electoral changes is rendered by Howard Ball, Dale Krane, and Thomas Lauth. The Voting Rights Act applies a "bottleneck principle" by requiring that all state and local electoral changes receive Justice Department or judicial preclearance. The department attempts to involve local black political leaders in the preclearance process, thus forcing white political leaders to negotiate electoral changes with local blacks. But the authors raise very serious doubts about section 5 enforcement: many electoral changes are apparently never precleared, many are submitted so close to elections as to preclude thorough review, and many are so vigorously pressed in every detail that Justice's objections are significantly eroded.

If there are disappointments in this volume, they are a weak chapter that fails to persuade that discrimination against blacks has deprived them of public benefits and a chapter that argues for such alternatives as proportional representation and add-on voting, which are at such odds with U.S. practice as to be implausible.

On the whole, however, this volume combines outstanding scholarship in diverse disciplines with forceful advocacy that has already had a major impact in reshaping Supreme Court doctrine.

DAVID ADAMANY

Wayne State University

Stability and Change in American Politics: The Coming of Age of the Generation of the 1960s. By Michael X. Delli Carpini (New York: New York University Press, 1986. xxv, 374p. $40.00).

The ambitious goal of this book is to estimate the effect of "the sixties" (here defined as the years 1963–1974) on mass political attitudes and behavior, especially on the attitudes and behavior of the "sixties generation." As such it is, in its author's words, "a detailed case study of the political impact of generational replacement" (p. xxi). Further, Delli Carpini wants to argue, first, that during the sixties values and ideas were diffused from the peripheral to the dominant subcultures in the United States (a reversal of the normal process); and, second, that the generational change represented by such value transformation has not persisted because it was not accompanied by structural or institutional changes.

In addressing the "impact" question, Delli Carpini is faced with the traditional problem of separating generational, period, and life-cycle effects on aggregate measures of attitude (or, more correctly in this case, of opinion). His solution is to use dummy variables to indicate membership (or nonmembership) in a generation and to indicate the potential presence or absence of period effects. Ordinary least squares analysis is used to estimate the effects of the three factors (generation, life-cycle, and period) on mean responses (of Michigan survey respondents) to a number of survey questions (I confess to a desire to see percentages here: what exactly is the difference between the sixties generation and their elders on measures of efficacy, political interest, and so forth? One might note also that constructing scales, rather than analyzing individual items, would have produced greater reliability and more concise analysis).

The resultant findings are interesting, if not surprising. For example, the sixties generation is more "liberal" than other generations, although between 1952 and 1980, period effects and life-cycle effects were pushing them (and the population) in a conservative direction. In terms of partisanship, the sixties generation is a "generation without a party"; they tend also to be less psychologically involved in politics. These descriptions of the sixties generation and of their impact on the aggregate changes in partisanship and participation should be familiar from the Jennings and Niemi studies and from such survey-based research as that of Nie, Verba, and Petrocik's *The Changing American Voter.*

A major problem with this analysis lies in its definition of generations, particularly the sixties generation. The solution chosen by the author to the identification problem assumes that one either is or is not a member of a particular generation; this means that assignment to a generation is critical. The categorization of all individuals who were between the ages of 1 and 35 between 1963 and 1974 as part of the "sixties generation" is far too broad. Moreover, this group is differentiated only by age (younger members are assumed to be "socialized by the sixties," whereas somewhat older members "experienced" the period, and the oldest are assumed to have been "ambivalent"). It is odd that despite the discussion of Mannheim, one of his central concepts, that of generational "units," is not used here. Mannheim's notion was that different groups (those in different socioeconomic positions, for example), would "work up" their common generational experience in different ways. We know that some of George Wallace's strongest support in 1968 came from those in their twenties; that only a small minority of college students protested the Vietnam War; that many people who came of age in the 1960s did not share the "culture of the sixties." Yet the present analysis essentially treats all those of a similar age as being of a similar political mind.

This problem is related to the lack of even an implicit theory of political learning. There is talk of generations being "imprinted," but no real discussion of how this occurs; the role of parents, for example, is pretty much ignored. The author goes so far as to say that even if experiences shape the particular political "character" of one generation, "nothing about the development of that generation allows one to say much about the character of the . . . following generation" (p. 62). This is true only if parents play no role in socializing their children. (Paul Beck's generational theory of partisan change is not cited here but is quite relevant.)

Finally, and disappointingly, the book fails to offer evidence in support of the two arguments proposed initially, those relating to the peripheral and dominant subcultures and to institutionalization of value change.

KRISTI ANDERSEN

Syracuse University

Environmental Law and American Business. By Joseph F. DiMento (New York: Plenum Press, 1986. 228p. $29.50).

Making Bureaucracies Think: The Environmental Impact Statement Strategy of Administrative Reform. By Serge Taylor (Stanford, CA: Stanford University Press, 1984. 410p. $29.50).

If the significance of new public policies were measured in terms of visibility, then the National Environmental Policy Act (NEPA), signed into law by President Nixon in January of 1970, certainly would be considered a great success. This federal legislation has been subjected to microscopic analysis by dozens of policy scholars over the last 16 years, and it has prompted the more conservation-minded states to enact similar legislation at the state level. Even some nations abroad have copied it. The basic question remains, however: has this highly visible and minutely studied legislation changed anyone's behavior (other than the behavior of "NEPA specialists")? The two books reviewed here, like others before them, seek answers to this knotty question.

Environmental Law and American Business and *Making Bureaucracies Think* read well in tandem. Professor DiMento's research encompasses not only NEPA but other state and federal antipollution legislation that followed, for the most part, in NEPA's wake. As the title of his informative book suggests, DiMento selects as his central focus the impact of environmental laws on private sector behavior—on how industries, both large and small, have complied with the numerous conservation-oriented regulations now on the books. In contrast, Taylor's scholarship focuses more exclusively on NEPA. His primary concern is with its impact on public sector behavior. Thus, read together, one is rewarded with a quite comprehensive treatment of the extent to which both the public and private sectors have complied with the legislation of the "environmental decade." Both authors likewise agree that there exists a considerable gap between promises and performance.

DiMento's book will interest both public policy scholars attentive to the intricacies of implementation, and practitioners, who have to find ways and means to comply with an expanding universe of governmental regulations. As the author says in the preface, "This book offers policy reforms that aim to achieve the goal of inducing business to comply with reasonable environmental law" (p. viii). After a nice introductory chapter in which he describes four cases of noncompliance ranging from the trivial to the life-threatening, DiMento gets to the crux of the issue: "The problem of violations of environmental law is immense" (p. 20). It is immense both because of the nature of environmental regulations and the possible costs involved. "The EPA estimates that the cost to comply with the Clean Water Act will be $118.4 billion by the year 2000" (p. 30). Add to that figure the costs for air pollution reduction, toxic waste cleanup, and so forth, and it becomes abundantly clear why there is so much foot dragging in the private sector when industries are asked to clean up their acts. Not only is it expensive, but the scope of environmental legislation has become so all-encompassing that it is a practical impossibility to uniformly force compliance.

Most of DiMento's book is, therefore, a description of (1) the various tools the government has at its disposal to effect compliance and (2) a detailed analysis of strategies used by industry to get around compliance and the reasons for adopting them. The first issue can be summarized as the "three *C*s" of compliance: "Government can court, coerce and cajole" (p. 65). It can bring down the full weight of the criminal justice system on noncompliers and/or it can sit down at the bargaining table and hammer out a reasonable compromise. Generally, the latter is to be preferred over the former. And concerning the second issue, industries do not comply with environmental laws for a wide variety of reasons. They utilize all sorts of guerilla warfare tactics to get around them. But not infrequently, says DiMento, the "dilemmas of compliance" with environmental laws hinge on the most basic of reasons—people do not understand them. "Among the most unresponsive [businesses], even to informal and personalized attempts to promote compliance, are dry cleaners, service stations, and water heater manufacturers. . . . Many of the employees of these firms do not speak English . . . and clearly do not understand the letter and spirit of some environmental rules" (p. 156).

Serge Taylor's book is also about compliance, but the drama now takes place within the federal bureaucracy and not in corporate boardrooms or under grease racks. Much has been written about this subject, and Paul Culhane has described *Making Bureaucracies Think* (appropriately, I believe) as the last published of first-generation NEPA studies. Certainly it will be of interest to NEPA scholars; its 408 pages contain a blow-by-blow description of how two of the most powerful federal agencies—the U.S. Forest Service and the Army Corps of Engineers—have proceeded in implementing NEPA. Generally, Professor Taylor's research corroborates what previous studies have found: Both of these federal agencies made serious and sustained efforts at incorporating environmental values into their activities, though not without some backsliding. Agency behavior has been modified as a result. "There is no question that all districts and forests do better in terms of avoiding environmental damage than they did before NEPA" (p. 130).

But in my opinion the book is more interesting, and certainly more provocative, in its effort to identify environmental issues with what Taylor calls "The Science Model." As the title of his book suggests, Taylor equates the pre-NEPA bureaucracy with a thoroughly politicized bureaucracy, one that paid precious little attention to the disinterested values of science and reason in carrying out its activities. Enter NEPA, the science of ecology, and the "environmental decade." In Taylor's view, environmental analysis—and environmental analysts working within agencies—had the effect of introducing scientific norms and values into federal agencies.

There are many problems with Taylor's analytical framework. I can discuss only the most important ones here. First, he ignores the classic work by Max Weber on the nature of bureaucracy, work that postulates a close correspondence between the professionalism embodied in bureaucracy and rational-scientific values. (The only reference to Weber is in a quotation from sociologist Arthur Stinchcombe, critiquing his *Economy and Society.*) We are all well aware that the U.S. variant of bureaucracy is more politicized than its European counterparts, but that is still far from arguing that U.S. bureaucracy is totally devoid of scientific, professional norms. Taylor comes close to saying just that.

Second, the author assumes that a concern with environmental values is by definition scientific. Many people—including this reviewer and some officials in the agencies he studied—would disagree. In their view, NEPA introduced a new set of interests, and perhaps even a new perspective, into agency activities, but environmentalism was as much a political movement as it was an embodiment of the scientific method and rational values. In fact, the science of ecology—of interrelationships, interdependencies, and impacts—is in its infancy. Even Taylor admits that "assessing environmental impacts is uncommonly difficult" (p. 17). When compared with an agency's traditional areas of expertise—in these cases, the science of silviculture and the applied science of civil engineering—ecology's scientific credentials fall far short not only of Taylor's hypothetical "Science Model" but also of these scientifically grounded and respected disciplines.

Taylor's airtight science-politics dichotomy gets him into trouble in this book as he attempts to argue that environmentalism is science and bureaucratic behavior is politics. What the book actually documents, as do others on the subject, is that the science of ecology didn't alter agency behavior; rather, it was the political pressures brought to bear by environmentalists—in the courts, in Congress, in presidential administrations, and in the press—that prompted recalcitrant bureaucrats to see the light.

JEANNE NIENABER CLARKE

University of Arizona

Politics and the Cinema: An Introduction to Political Films. By Michael A. Genovese (Lexington, MA: Ginn Press, 1986. viii, 124p. $17.96, paper).

Political scientists who make use of feature films and documentaries in their courses or who are simply film buffs with a special interest in films that deal with political topics will welcome this slim volume. The author is a political scientist who loves movies and teaches a course on political films.

Genovese considers films as tools of socialization and therefore considers all films political because of what they say or fail to say. After a brief description of the rise of the film industry, he deals with such topics as censorship, the Hollywood blacklist of the 1950s, political dramas, comedies, war and antiwar films, the specific contributions of several major U.S. and European directors and the historical images of men, women, and blacks, primarily in U.S. feature films.

There are two principal problems with this book. One is derived from Genovese's insistence on a major theme, and the other may stem from the volume's brevity. These liabilities damage the book severely.

His major theme is that "most [U.S.] films support and glorify the status quo," and he asks, Why would the wealthy "finance the making of films designed to overthrow the system which protects their wealth?" (p. 3). He misses the point. Hollywood aims for formula mass entertainment and is quite willing to package antiestablishment films if the market is there. Indeed, when he uses illustrations of political dramas, he cites *The Graduate, Easy Rider, Medium Cool,* and *The China Syndrome,* all distributed by mainstream studios, and all critical of the "system." He could also have noted that in the 1960s and early 1970s there were such films as *Getting Straight, The Strawberry Statement, Woodstock,* and *Alice's Restaurant,* all major studio efforts, and all antiestablishment. If the genre petered out, it was strictly a box office judgment. Powerful (and profitable) films that purport to hold a mirror to the nation's shortcomings are not all that rare: *Bonnie and Clyde, Chinatown, The French Connection, Silkwood, The Verdict, Birdy, The Killing Fields,* and *The Color Purple* certainly fit. Nor does he note such significant films of the old studio days on racial and religious tolerance as *Crossfire, Gentlemen's Agreement, Home of the Brave, The Defiant Ones,* and *Bad Day at Black Rock.*

In his brief discussion of the 1930s, the author insists that the film industry engaged primarily in ostrich cinema, virtually ignoring the depression. Yet he fails to acknowledge such significant problem films as *An American Tragedy, I Am a Fugitive from a Chain Gang, Black Fury, Winterset, Dead End, One Third of a Nation, The Informer* and *The Life of Emile Zola.* He touches on the clumsy portrayal of politicians in U.S. films, but ignores *The Best Man* and *The Candidate.* His concern for the western as political metaphor is limited to consideration of several John Ford films, slighting such achievements as *High Noon* and *McCabe and Mrs. Miller.*

Genovese's politics are intrusive. He repeatedly and justifiably denounces the impact of McCarthyism on the nation and the arts in the 1950s, yet when he quotes Charlie Chaplin's *Monsieur Verdoux* to the effect that business is murder and murder is only a form of business, he describes the film as Chaplin's "most biting, hardest hitting piece of social criticism" (p. 37). When he notes that the Emile de Antonio documentary *Millhouse* "creatively" intersperses different quotes from Nixon, he states that the power of the film is "indisputable, even if its method *may* be biased" (p. 55, italics mine). The 1982 film, *Missing,* directed by Constantin Costa-Gavras, merits particular attention. Despite the absence of any evidence, the film charges that a young U.S. citizen was killed during the overthrow of President Allende in Chile in 1973 with the knowledge of, and probably with the collusion of, U.S. officials. Genovese concedes that "this film takes liberties with some of the facts" (p. 87), but he neglects to inform his readers that the message on the screen at the beginning of the film states, "This film is based on a true story. The incidents and facts are documented. . . ." And his concept of judicious evenhandedness leads to the following: "In the U.S. conglomerates have enormous control over which films are or are not made. In the Soviet Union, it is the state which determines what films will be made. The result in both countries is clear: often we get propaganda" (p. 90). Presumably we can anticipate the Soviet equivalent of *Apocalypse Now,* and it will be about a portly Russian colonel sequestered in an Afghan cave.

Then there are the editing problems. The author tells the story of the Department of Justice attempt to label a Canadian film as propaganda twice within eight pages (pp. 56, 64). His quote from Michael Corleone of *The Godfather* to a U.S. senator, "we're both part of the same hypocrisy," is invoked twice, in consecutive paragraphs (p. 76). His section on D. W. Griffith is placed between sections on John Ford and Francis Coppola. The book is also marred by far too many typos and page 83 of the reviewer's copy was a total blank; and there is no index.

In his preface, Genovese asks if the subject of political films merits an entire book. It most assuredly does.

SIDNEY WISE

Franklin and Marshall College

The Democratic Imagination in America: Conversations with Our Past. By Russell L. Hanson (Princeton: Princeton University Press, 1985. xi, 478p. $45.00, cloth; $12.95, paper).

It is a pleasure to praise this book for the insightful way it traces the development of competing conceptions of democracy in the United States, for its richness of detail, and for the manner in which it traverses the intellectual space that has come to separate the humanities from the social sciences. These are accomplishments of high order, and they owe much to the fact that the author grounds them in the interpretivist and critical theories that spring from the writings of (and the debate between) Habermas and Gadamer.

In the intellectual territory that now divides the humanities from the social sciences, two preoccupations rise high on the landscape. One has to do with the uses of the past. And the other is an awareness (as a literary scholar recently said) that those who study human affairs cannot escape their own involvement with the subject that they study.

Humanists, traditionally, have accepted this involvement openly and cheerfully, using their own humanity to discover what Tocqueville once called "habits of the heart" among those they study. In so doing, they become confident of their ability to move outward from a single case to the whole of the human condition. For most social scientists, accepting their own involvement with what is studied comes far less cheerfully. How, we ask, can we avoid the contamination of our own values? And—even more painful to ask—can reality be other than a social construction? Sometimes, we avail ourselves of Weber's *verstehen* or Popper's situational logic. But with notably few exceptions, we prefer to seek the objectivity that is thought to be safeguarded by an aggregation of data and a sufficiency of comparable cases.

With respect to uses of the past, most social scientists take it as a given that the past is to be used to inform the present—to help us understand how the present came to be. But for humanists the premise is far different. The present is to be used to inform *on* the past, each generation of scholars taking on itself the obligation to use contemporary sensibilities to reinterpret the past. As a distinguished historian of art put it, the humanist's highest achievement comes when he can "look with fresh eyes" upon a subject that has become "tarnished with familiarity."

It is within these humanist traditions that Hanson holds his "conversations with our past," interposing his own definition of contemporary sensibilities between the written and verbal exchanges of those who have been important contributors to the ideas (and ideals) of U.S. democracy—all the while working "outward" from holographics to the whole of an era's political thought.

History is Hanson's organizing framework and the reader thus moves through periods that can carry familiar labels: the "Time of the Founders," the "Age of Jackson," and so on. But almost always, the author manages to look with fresh eyes upon those times, for he writes with a sensibility that is informed by the textual deconstruction of contemporary literary studies and with a sense of contemporary linguistic archaeology, peeling back layer after layer of word meanings until the terms of politics stand revealed as legitimators of an era's dominant social and economic structures.

Hanson rejects the convenience of a transcendental history, especially the idea that ours has been a progression from grudging and fragmented beginnings toward some richer and better version of democracy. If anything, according to Hanson, the development of the democratic idea in the United States has been more regress than progress. At the founding of the republic (understandably, given the social and economic class of the founders) the idea of democracy carried with it a concern for limited government, the obligations of civic virtue, and the expectation that private desires be balanced against the common good. But on Hanson's showing, each succeeding period altered that expected balance, until, in the post-New Deal period, the idea of democracy was transformed into a democracy of "rights-bearers," a democracy whose politics has come to turn on the right to material entitlements and the satisfaction of consumer wants.

There are arresting omissions and commissions in Hanson's account. No mention, for example, is made of Thoreau and only passing reference is given to Lincoln, but there are several pages of attention to the futurist novels of Ursula K. LeGuin, the student movements of the 1960s and seventies, and that era's prophet of new consciousness, Charles Reich. But we may presume that the commissions (along with the omissions) contribute to Hanson's larger purpose: to argue on behalf of an alternative future that would bring to U.S. democracy a politics of moral concern grounded in opportunities for enhanced citizen participation in life-affecting issues and for living lives that are satisfyingly rich in nonmaterialist concerns.

LAWRENCE J. R. HERSON

Ohio State University

Running for Office: The Political Education of Campaigners. By Marjorie Randon Hershey (Chatham, NY: Chatham House Publishers, 1984. xii, 306p. $12.95, paper).

Can one book say something new and significant about congressional elections, political parties, political action committees (PACs), "single-issue" groups, and representative government? This one does. In a departure from the literature on campaigns, Marjorie Hershey focuses not on what candidates do, but on how, what and from whom they learn. Her theme is that campaigns "leave their mark on American politics less by changing the officeholder's face than by influencing the officeholder's thinking—and the perceptions and behavior of challengers, activists, reporters and others as well" (p. 31).

For candidates, the political classroom of the 1980s is a tough and unruly place. Hershey sketches brilliantly the new campaign environment, discussing campaign finance, PACs, single-issue activism, the Christian Right, campaign technologies, the media, and the "declining" political parties. All these combine to make the candidate's lessons more complicated and difficult than ever.

How do campaigners cope with such an environment? Drawing on social learning theory, Hershey outlines several hypotheses about how candidates learn, whether from previous experience, observation of others, or models provided by outside experts. Especially provocative is her contention that parties—or at least the GOP—may be reasserting influence by providing campaign models and assistance. Still, as the author concedes, the Democrats, as well as state and local units of both parties, are ill equipped to assist candidates, especially in competition with interest groups, PACs, and campaign consultants.

Hershey tests the hypotheses about political learning by looking at the 1980 campaigns of six liberal U.S. senators targeted by antiabortion groups. Drawing on extensive interviews, newspaper research and personal observation, she confirms that both candidates and activists are constantly learning, that they look to appropriate models, primarily among fellow partisans or issue enthusiasts, and that they often modify strategies in response. But what they learn is problematic. Voters provide little direct feedback during the campaign, and election outcomes are "blunt instruments" for conveying what citizens really want. Indeed, only the postelection "battle of constructed explanations," dominated by political and media elites, finally crowns the victor in the war of political meaning.

Campaigns, it would seem, make only a modest contribution to representative government. Not only do elections change few faces, they often fail to attune candidates to the real concerns of voters. As Hershey notes, "the learning environment of an elected representative systematically amplifies the voices of organized groups, other officeholders, party leaders and media figures" at the expense of the "quiet citizen" (p. 269). She offers some ways to clarify the electorate's muffled tones, such as enhancing electoral competition—perhaps through public financing of campaigns—and increasing the availability of poll information, but these seem unlikely to effect major changes in candidate learning.

Despite Hershey's convincing analysis, the reader should be cautious about generalizing too broadly from a case study. The political environment of the late 1970s and early 1980s may be atypically unstable, making candidate learning unusually difficult. In addition, Hershey's conclusions about single-issue activism are tied too closely to one case, the struggle over abortion. In fact, single-issue activists and strategies defy easy characterization. And, of course, races for other offices occur in markedly different environments. All these issues require further attention, but future studies of candidate learning will, we hope, match the originality and insight of this one.

JAMES L. GUTH

Furman University

Governing Buildings and Building Government: A New Perspective on the Old Party. By Bryan D. Jones (University, AL: University of Alabama Press, 1985. xvi, 230p. $27.50).

Jones's book is a case study of Chicago's Department of Buildings. The book's subtitle, *A New Perspective on the Old Party,* reveals a central theme of the volume—that Cook County's Democratic party is involved in most aspects of the city's handling of the built environment, ranging from the policy arena in which building codes are developed, to the administrative arena in which building regulations are enforced, to the Housing Court in which violations are adjudicated. At first blush, one might wonder whether study results showing the centrality of the Democratic party in a particular area of Cook County governance are superfluous. They are not, for several reasons. For one thing, there is remarkably little empirical evidence of party influence in the Chicago urban service delivery process, despite widespread beliefs about the party's role in this regard. Ken Mladenka's recent study of the distribution of parks and recreation, fire protection, education, and refuse collection services in Chicago, for example, left scholars with quite the opposite conclusion—that is, that "distributional decision making is routinized and largely devoid of explicit political content" (The Urban Bureaucracy and the Chicago Political Machine: "Who Gets What and the Limits to Political Control," *APSR* 74(1980):991–98). If such studies raise doubts about the relevance of party considerations in service distribution, parties' alleged disdain for matters of policy formulation is an even more widely accepted truth, perhaps best stated in Raymond Wolfinger's piece, "Why Political Machines Have Not Withered Away and Other Revisionist Thoughts" (*Journal of Politics* 34(1972): 365–98).

Jones's empirical analysis is most distinctive and challenging to existing work in its finding that the Cook County Democratic party exerts substantial influence over distributional outcomes at many points in the building code enforcement process. Of particular interest are findings that the strength of ward organization significantly affects not only the level of citizen demands and the seriousness of bureaucratic response, but also the extent of compliance. Using a skillful combination of quantitative data analysis and qualitative case study materials, Jones shows *how* the party manages to influence bureaucratic outcomes through a system of "attention rules" based on strength of ward organization, existing alongside the usual "bureaucratic decision rules" based on professional, technical criteria.

At first, one expects that this volume will offer an equal counterpoint to the notion that urban political parties (at least insofar as they fit the machine politics model) are largely uninterested in substantive policy formation. But Jones's chapter on substantive changes in the building code is more ambivalent on this matter. Jones applies the corporatist concept to analysis of building code policymaking, thereby emphasizing that this is a structured and relatively exclusive policy arena rather than a freewheeling, pluralist one. Within this corporatist model, the interests of city bureaucrats are brokered along with those of major private interests such as architects and building trade unions. But, as Jones concludes, "the party itself has no interest in substantive policy making. Rather, it acts as a resource for cementing the policy system and for ensuring that the participants in the process automatically take into consideration the policy preferences of the mayor" (p. 38). While the distinction between the policy preferences of the mayor as a governing official and the resources and interests of the mayor as a party leader is a valid one, one wishes that the possible connections among these were given greater attention in this chapter. Nevertheless, it is a rich and lively one, concluding with a look at the city's new rehabilitation code—a policymaking "incident" that contrasts with the corporatist model and serves nicely as the exception that proves the rule.

Despite its main focus on the role of the party, the most revisionist aspect of this book may be its treatment of management control. Views of the "inherent" discretion of street-level workers in urban bureaucracies are widespread, and the corruption incidents that periodically engulf Chicago's Department of Buildings might seem to corroborate the idea that street-level discretion and its abuse is endemic. But Jones shows how bureaucratic responses to scandal, involving classic management control devices, can significantly constrain street-level discretion, albeit with some unintended costs. In fact, Jones's analysis of Chicago's response to periodic corruption scandals yields an intriguing model of a two-track policy system, one track dealing with substantive policy issues and the other with periodic management control crises.

In short, Jones's volume continues a long and distinguished tradition by which the city of Chicago serves as a laboratory for students of urban politics. It is a fine example of the descriptive richness and theory relevance that a good case study must have.

ELAINE B. SHARP

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Novus Ordo Seclorum: The Intellectual Origins of the Constitution. By Forrest McDonald (Lawrence: University of Kansas, 1985. xiii, 359p. $25.00).

I suspect the next two years will see a rash of books published to commemorate the bicentennial of the Constitution. Forrest McDonald is an early entrant in this field. McDonald sets out to "make a reasonably comprehensive survey of the complex body of political thought (including history, law and political economy) that went into the framing of the Constitution" (p. ix). After laying out the many strands of political thought current in the United States in 1787, McDonald examines the use the framers made of political theory in the Constitutional Convention. He asserts that "the political theories and ideologies at the command of the Framers were, as we have seen, of limited practical use" (p. 26). McDonald's thesis is that the Constitution is yet another example of political actors who selectively use political theory and ideology to justify decisions made on more pragmatic grounds.

McDonald's contribution to this familiar thesis is to argue that the transitions that political theory, economic theory, and jurisprudence were undergoing in the late eighteenth century made it easier for politicians to pick and choose from the numerous theories according to their political need. This, in turn, expanded the theoretical possibilities through the combination in practice of what had previously been theoretically distinct. For example, McDonald shows how the United States in the post-Revolution era passed mercantilist measures such as protective duties on imports at the same time they advocated increased competition in the shipping industry. In another example, McDonald contrasts the experience of the States with test oaths, sequestration acts, installment laws, and paper money with that of the republican belief in the state support of public virtue as evidenced by the passage of sumptuary laws.

Unfortunately, McDonald's book reads as if it were two. The first part is a book on the intellectual history of the ideas current with the framers. Here he provides a survey of the key theoretical concepts in the 1780s, such as property rights, republicanism, public virtue, and commerce. McDonald painstakingly documents the multitude of meanings these concepts took under different authors such as Hume, Mandeville, Bolinbroke, Montesquieu, Quesnay, and Smith. He records an exciting time when major changes were occurring in political, economic, and legal theory. McDonald focuses on property rights and the significance of the change in economic theory, started by Mandeville and Smith, that promoted the beneficial effects of an unrestrained commerce. He contrasts this with the political theory of classical republicanism that argued for the necessity of public virtue to the survival of republics. He concludes that no consensus existed that the framers could draw on in designing the new constitution.

McDonald next turns to the use of these eighteenth-century intellectual controversies by eighteenth-century politicians. This is the second part of McDonald's work. His thesis in this part of the book is that politicians exploited the lack of consensus in theory to develop hybrids of the various theories according to their political needs of the moment. He presents case after case where politicians in the states in the postwar era used theory to suit and justify their ends. McDonald argues that the experience of the states caused for many a "reconsideration [of] their ideas about republican forms of government and about safeguards to liberty and property" (p. 178). The lessons of this reconsideration only reinforced the diversity of opinion.

So, finally, when McDonald turns to look at the decisions made at the Constitutional Convention, he finds little influence of political theory in the design of the Constitution. Part of the blame lies in the fact that the framers came to the convention with theories of politics that were diverse and often inconsistent. The rest lies in the pragmatic use the framers made of theory to justify their positions reached from other motives. The framers, McDonald writes, "used political theorists to justify positions that they had taken for nontheoretical reasons" (p. 235). Yet he lauds the outcome, the Constitution, as a significant accomplishment for republican theory. Frankly, it seems to be a variation of the story about the emperor who has no clothes where the tailor is given an award. How can that be? McDonald fails to carry through with his analysis using the categories of theory he distinguished in the first half. For example, McDonald discusses the use of the various theories of republicanism by the framers. But, instead of using the categories arrived at in his earlier chapter, he creates new categories of division and bases his analysis on them. As such, his examination of the influence of republican theory in the minds of the framers is divorced from his early discussion of its origins. Further, his analysis of the decisions made in the convention often default to routine listings of the time sequence of events. It is no wonder, then, that McDonald finds so little influence of political theory in the design of the Constitution.

Although I suspect the book was written for a general audience, it is difficult to read. McDonald's approach to the complexity of the subject is to make a series of nested lists detailing the various distinctions in the intellectual origins of the ideas. The book's aim is too comprehensive to be enjoyable. It would better serve as a reference book on the intellectual precedents to the Constitution. McDonald's development of the history of the idea of an unrestrained commerce and its relationship to republican theory was the liveliest part of the book. This carries over to this examination of the origins of the contract clause in the Constitution. It is unfortunate that McDonald did not confine his work to that area.

EVELYN FINK

Dartmouth College

American Prince, American Pauper: The Contemporary Vice Presidency in Perspective. By Marie D. Natoli (Westport, CT: Greenwood Press, 1985. xiv, 204p. $35.00).

Marie Natoli's characterization of the U.S. vice president as both prince and pauper highlights the distinctive awkwardness of that office. The vice president is prince by virtue of being first in the line of presidential succession, and in the last forty years such succession has taken place three times (FDR-Truman, Kennedy-Johnson and Nixon-Ford). Other than the possibility of succession, however, the vice president is pauper in that the office lacks substantial authority of its own and is entirely dependent on the president to give it significance. So many presidents in the past have assigned their vice president so little to do that the post gained a reputation for obscurity and for being a dead end for political careers.

Natoli's study tracks the emergence and development of a more visible and vigorous vice presidency in the post-World War II period to the present, from Harry Truman to George Bush. Emphasis is given to the greater political attractiveness of the office, as evidenced by the willingness of major leaders to accept the vice presidential nomination, of presidents to make more effective use of their vice presidents, and of vice presidents to seek and to be expected to seek the presidential nomination of their party. Unlike some other commentators, Natoli does not term these changes a transformation of the vice presidential office. Nonetheless, she treats the uptrend as an enduring development that has considerably strengthened the place of the vice president in the political system.

Such judgments seem premature not only because the evidence is mixed and uneven but because the generic constrictions on the vice presidency remain unchanged. What the vice president does in office, as Natoli sensibly stresses, continues to be a function of what the president permits and wishes. Hence the Carter-Mondale experience, considered by many to be the contemporary highwater mark in effective presidential-vice presidential relations, may or may not turn out to be a precedent future presidents will want or feel obliged to follow.

The vice president's gain in prominence necessarily takes the form of a more active role as an agent of the president. As a consequence —and Natoli underscores the point—severe constraints and vulnerabilities exist. When acting for the president, the vice president may have to advocate personally uncongenial policies, develop an image helpful to the president but not necessarily to him- or herself, and often handicap as much as assist future prospects and ambitions. These difficulties are most pronounced for a vice president who pursues the presidential nomination. The incumbent vice president can not readily reject current policies or espouse new ones at marked variance from those of the president without inviting party disunity and opening him- or herself to charges of personal disloyalty, lack of credibility, and untrustworthiness. The heightened political visibility of the modern vice president can come, then, at very high political cost to the incumbent's presidential prospects, as was the case with Hubert Humphrey and Walter Mondale. It remains to be seen whether George Bush's faithful service to President Reagan will constitute, on balance, a plus or a minus in his quest for the presidency in 1988.

Students of the vice presidency should find Natoli's account helpful in its descriptive coverage of actual vice presidential experiences since 1945 and in its sensitivity to the tensions inherent in the office and in its relationship to the presidency. The study would have been stronger had Natoli analyzed more fully her own proposals for change and those of others, including her dismissal of the Twenty-fifth Amendment as having "little merit" (p. 91).

ALLAN P. SINDLER

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The Life of the Parties: Activists in Presidential Politics. Edited by Ronald B. Rapoport, Alan I. Abramowitz, and John McGlennon (Lexington, KY: University Press of Kentucky, 1986. 242p. $24.00).

The Life of the Parties provides an interesting and informative examination of a relatively neglected set of political actors—state party convention delegates—and makes an important contribution to the growing debate over the general decline of the U.S. political parties thesis. It is based on surveys of both parties' state conventions in 1980 in 11 states.

The heart of the book is found in chapters 4 and 5, which were previously published in the *Journal of Politics* and *APSR.* In chapter 4 the editors argue that partisan as well as purposive (candidate and issue support) motivations were operative, that the two types of motivations were interrelated rather than mutually exclusive, and that partisan motivations depressed support for Kennedy among liberal Democratic activists. While most would agree with the conclusion that partisanship was an important motivation among state party convention delegates, differences between motivational groups are clearly evident as party identification and political experience variables are more strongly related to partisan than purposive motivations. Perhaps in a more evenly divided Democratic convention without an incumbent president running for reelection, purposive motivations would have played a more important role than in 1980. The chapter, nevertheless, is an important contribution to the literature, which heretofore has relied heavily on the 1972 national Democratic convention, a convention dominated by liberal amateurs.

Chapter 5 by Abramowitz and Walter Stone is an especially excellent piece that ultimately employs recursive path analyses to examine whether delegates were more motivated to support candidates whom they perceived as ideologically closest to themselves or those they perceived as most electable in November. While the authors conclude that electability was more important than ideological proximity continued to exert a very significant total effect on preference. An especially nice addition involves an analysis of a sample of 1972 Democratic national convention delegates, which results in a revision of the literature to reflect the greater direct effect of electability than ideology on candidate preference. I would caution, however, that ideology may well have been more important in 1972 than eight years later, even for the same individual delegates. While the authors stress that party activists are primarily motivated by *perceived* electability, this does not necessarily contribute to strong parties seeking moderate candidates most likely to win, since delegates' perceptions of reality may be incorrect, and reciprocal but untested linkages may exist since delegates' candidate preferences may shape their perceptions of the candidates' electability.

Other chapters also provide valuable information and insights. Chapters 1 and 2 provide a nice introduction to the mechanics and scholarly literature on the presidential nomination process in general. In a descriptive analysis of the data set, the editors in chapter 3 document the relatively high socioeconomic status of party activists, their intense partisan identification, and the significant ideological and issue differences between the parties in each state. While such basic descriptive information is helpful and necessary, a more analytic approach comparing this group of activists with the state population and with other activists in regard to these characteristics would have been especially interesting. Chapter 7, by Laurence Moreland, Robert Steed, and Tod Baker, provides an interesting analysis of the impact of migration on the state parties, discovering that Democratic newcomers to a state were more liberal than longtime residents, largely because they were younger and better educated. This suggests that some of the same factors found by Everett Ladd and Charles Hadley to contribute to the ideological polarization of the parties, such as a college education, continue to operate and that our increasingly geographically mobile society may introduce another challenge to party leaders. Chapter 8 by Ronald Rapoport will be of interest to the numerous scholars who follow the issue constraint debate, as he examines issue constraint among party activists and finds constraint highest among the college-educated and those motivated by ideological concerns.

The remainder of the book contains considerable information, though it is not sufficiently guided by theoretical concerns. Chapter 6 examines the interest group associations of each party's convention delegates, chapter 9 employs factor analysis to identify the belief systems of each party and various groups of delegates, and chapter 11 finds that few of the delegates were ideologically extreme and that even this small group did not significantly differ from other delegates in other important respects. Chapter 10 addresses the time-honored theme of the permeability of the parties, pointing out that Democratic delegates who had switched to that party in the 1960s were especially well educated, liberal, and attuned to the issue concerns of that decade.

This book will be of special interest to anyone specializing in political parties and political behavior generally. There is a definite need for more study of such midlevel party activists, and the identification and explanation of longitudinal trends can only be achieved by replicating this study in future years.

STEPHEN D. SHAFFER

Mississippi State University

The Jesse Jackson Phenomenon: The Crisis of Purpose in Afro-American Politics. By Adolph L. Reed, Jr. (New Haven: Yale University Press, 1986. xii, 170p. $17.50, cloth; $5.50, paper).

Adolph Reed, Jr.'s *The Jesse Jackson Phenomenon* is among the first published of a number of works on the 1984 Jackson campaign for the Democratic presidential nomination. It is an explosive book destined to provoke debate and disagreement on Jackson himself, the appropriateness of a challenge for the presidency by a minister, the reality and the meaning of possible class differences among blacks and their support for Jackson, the significance of black concerns for the Democratic party, and the role and legitimacy of nonelected individuals and institutions. All of these are fascinating, complex, and important issues. Unfortunately, Reed's treatment of them is limited in two ways. He is right on a number of issues and he has interesting insights, but many of his hypotheses are left unsupported by data based either on his own research or on existing materials. Secondly, the analysis is hampered by Reed's unwillingness or inability to bring a reasonable form of discourse to his subject matter.

Reed is correct in saying that although the black church is often described as an aggressive initiator of protest during the civil rights movement, it can display socially controlling, conservative behavior. The church also provides a framework within which individuals reshape their social and political behavior and external world, as Aldon Morris argues in a recent work with which Reed disagrees. Research therefore needs to be conducted on how some churches or religious leaders are more liberal while others are more aggressively conservative and on the factors that provoke the shift from a social control to a social change mode by such institutions.

Reed puts the claims of the Jackson campaign under careful examination. He cites the prediction by Jackson's supporters that he would mobilize large numbers of black voters, thereby significantly improving the Democratic nominee's chances in 1984, but Reed concludes that Jackson's voter mobilization capacities were ambiguous at best. While he is accurate in noting the weakness of Jackson's voter organization and mobilization structures, the book lacks any estimation of the total black and total voter turnout actually generated by Jackson. Reed's failure to present this information denies him the opportunity to prove conclusively that Jackson did not generate substantial black participation during 1984. Secondly, he cannot verify his claims that Jackson's support among black voters was not mass-based. While data cited by Reed, as well as data in other surveys, show that upper-income blacks supported Jackson more strongly than lower-income blacks, the candidate won a majority of black voters in every economic category in every state except Alabama. Reed also uses the failure of Jackson's allies' campaigns as proof of his failure at mobilization. He might have made this point more effectively had he compared Jackson's black allies' and opponents' turnout and total vote with the election results of black candidates who did not win Jackson's support. Instead Reed provides us with examples of a few failures, but we have no idea of the universe of electoral contests nor of the total votes involved.

Reed's concern with leadership validation of the protest and electoral wings of black politics and with the intersecting problems of conflict between elite and mass political interest pervades the book. He argues that the protest wing has had to adjust to the existence of a new category of black political leaders in the post-civil rights era. Certainly adjustment and competition should be expected as new categories of political representatives arise, and procedures for legitimation of unelected interest groups are matters for concern, but Reed seems to think the latter lack validation simply because they are not elected. This is too facile. Reed should and does know better. The obvious implication following from this is that Jackson as a self-appointed leader had no previous electoral authorization and that his right to contend for the presidency is therefore in doubt. Reed's approach suggests that the only form of valid representation is through the electoral process. While electoral affirmation is critically important, the aggregation of political ideas, values, and concerns is accomplished in multiple ways including, but not limited to, competition for political office.

Finally, Reed must be faulted for his consistent lack of restraint. A sine qua non for serious work is rational discourse, but Reed too often succumbs to the cutting comment or the witty thrust instead of analysis, which suggests a basic lack of respect for his subject matter. He castigates those "unable to distinguish between a social movement and a group of people shouting in a church" (p. 12); he opines about "Pentacostals, Jehovah's Witnesses, and other flotsam and jetsam of the religious universe" (p. 58); he observes that "the [Jackson] campaign manipulated its adherents' fears and despair in much the same way as do the quick-fix purveyers of lucky rabbits' feet, millenarian cults and religions of positive thinking" (p. 37).

These observations do not mean that political scientists should not criticize the Jackson campaign or the candidate Jackson. It makes no intellectual sense to do so, however, unless the critical analysis adds to our understanding of politics in the process. There is much that is valuable in this book. Reed raises interesting and provocative questions but answers far too few of them convincingly. I expect he will do so in future work.

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The 1984 Presidential Election in the South: Patterns of Southern Party Politics. Edited by Robert P. Steed, Laurence W. Moreland, and Tod A. Baker (New York: Praeger, 1986. viii, 340p. $39.95).

For almost a decade, Robert Steed, Laurence Moreland, and Tod Baker have fostered the study of southern politics through their biennial Citadel symposia on southern politics and a series of edited books developed from these conferences. *The 1984 Presidential Election in the South* differs from their three previous studies in that it focuses on a single election and treats that election primarily on a state-by-state basis. Although the book begins with William Havard's judicious overview of southern political trends and concludes with Harold Stanley's insightful interpretation of the 1984 election for the entire region, the rest of the study consists of careful accounts of Ronald Reagan's easy victory in one southern state after another. Each of the state chapters summarize the state's recent political history, reviews nominating activities and the general election campaign, analyzes the popular vote, and discusses the implications of the 1984 presidential campaign for future partisan politics.

The editors' decision to use individual states as the primary unit of analysis inevitably leads to considerable repetitiveness, yet the experience of reading 11 accounts of the 1984 election does have the virtue of hammering home the magnitude of the Democratic collapse and the breadth of the Republican superiority in southern presidential politics. In recent presidential elections substantial majorities of white southerners have voted Republican and even larger majorities of black southerners have voted Democratic. Since there are four times as many whites as blacks in the southern electorate, it is not difficult for the Republicans to win presidential elections despite achieving only microscopic black support. Republican success is especially likely when (as in 1984) a popular conservative Republican president runs against an undisputedly liberal Democrat. Given southern white antipathy for the candidates, issues, and groups generally associated with the national Democratic party, William Stewart's conclusion for one state—"The votes may simply not be there to construct Democratic presidential victories in Alabama any more" (p. 87)—seems equally applicable to the other southern states.

All of the chapters are instructive. Dennis Ippolito offers a revealing account of the Texas Republicans' "victory of unprecedented and unexpected scope" (p. 159); Jack Fleer skillfully assesses the Republicans' presidential, senatorial, and gubernatorial victories in North Carolina; and Larry Sabato provides a thorough analysis of Virginia's "predictable results" (p. 276).

Although all 11 southern states awarded landslide votes to Reagan, considerable differences remain among the states in terms of Republican competitiveness in nonpresidential campaigns. Moreland, Steed, and Baker find the Republican party in South Carolina on the verge of major advances, as does Charles Hadley for Louisiana Republicans. In contrast, spectacular Republican gains in nonpresidential elections appear less likely in Stewart's analysis of Alabama, Thomas Walker and Eleanor Main's discussion of Georgia, Diane Blair's study of Arkansas, and the treatment of Tennessee by Anne Hopkins, William Lyons, and Steve Metcalf. One of the book's strengths is that contributors were encouraged to pursue the themes they considered most relevant to understanding southern party politics. As a consequence, Alexander Lamis emphasizes the salience of racial cleavage in Mississippi politics, while Mark Stern stresses south Florida's prominence as the locus of advancing Republicanism. The most ambitious chapter is Stanley's contribution, which argues that "Reagan's popularity and the Republican gains did not result primarily from white racial backlash" (p. 322) and that "Democratic dominance below the presidential level cautions against a sweeping conclusion proclaiming realignment in the South" (p. 324).

Students of southern politics will find *The 1984 Presidential Election in the South* a valuable exploration of southern Republicanism at its peak under Reagan's leadership and popularity.

EARL BLACK

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COMPARATIVE POLITICS

Chinese Marxism in Flux, 1978–1984: Essays in Epistemology, Ideology, and Political Economy. Edited by Bill Brugger (Armonk, NY: M. E. Sharpe, 1985. 218p. $30.00, cloth; $14.95, paper).

The Government and Politics of the PRC: A Time of Transition. By Jurgen Domes (Boulder, CO and London: Westview Press, 1985. xv, 316p. $42.50, cloth; $17.95, paper).

Institutional Reform and Economic Developments in the Chinese Countryside. Edited by Keith Griffin (Armonk, NY: M. E. Sharpe, 1984. x, 336p. $37.50, cloth; $18.95, paper).

The changes in post-Mao China have resulted in a plethora of studies addressing the actual implementation of policies and their apparent short-term results as China seeks to maintain socialism while reinstating material incentives and the pursuit of wealth. Emerging studies also reflect the increased possibilities for research in China. Field studies offer new opportunities to gather data, especially since cadres are more open in their discussions and data are more plentiful in Chinese publications.

The three books reviewed here are, to some degree, products of this new situation. The Griffin and Domes volumes are well served by the openness. Brugger is less beholden to the new data and relies more on the philosophical and ideological publications that have been integral to Chinese publishing since the early days of the People's Republic of China (PRC).

The Brugger volume consists of six papers drawn from conferences held in Australia in 1982 and 1984. The papers consider the Marxist theories that guided the pre-1978 leadership and how they have fared in the heady atmosphere of the post-Mao period. In the first chapter, Dutton and Healy argue that Mao never broke with the empiricism of Stalin, and indeed official Marxism has not yet done so. The current theories are rather a return to Stalin's empiricism, with the class struggle "collapsed into the economic instance" (p. 3). In the second chapter, Sullivan discusses the theory of continuing revolution. Brugger follows with an able analysis of the problems of extensive and intensive development. Next, Hannan argues that Marxist-Leninist theory is inadequate regarding discrete modes of production. Additional chapters deal with the continued strength of the state and the law of value. The arguments, though theoretical and sometimes abstruse, are surprisingly well joined by internal comments and cross-references. It is possible to follow what are often obscure arguments, but the volume is likely to be attractive primarily to Asian specialists.

The exceedingly interesting anthology edited by the distinguished scholar Keith Griffin is composed of papers authored by development scholars rather than area specialists, who draw on field visits to China in 1982. The extensive and informative charts in the book are based on data from communes in Yunnan and Sichuan that the authors visited, and they make use of other material as well. The volume commences with a discussion of institutional change and income distribution (Keith Griffin and Kimberley Griffin) and goes on to a detailed analysis, "The Responsibility System and Institutional Change," by A. R. Khan. Subsequent chapters treat issues of concern both to those interested in general economic and social development and to those focusing on China. In particular, the analysis of rural reforms will be of wide interest.

The chapters are well written, comprehensive, do not confuse anecdotal materials with evidence, and limit their claims to those suggested by the data available to them. To some extent, however, events have passed the authors by. Because they are development specialists, the writers tend to emphasize continuity in politics. They note the ways in which pre-1978 policies have been adapted and stress that a number of the current changes have deep roots in Chinese economic history. Some judgments do not stand up well. For example, in the discussion of whether conditions had improved, it seems to be assumed that the malnutrition they observed will continue. For the most part, however, the authors are judicious and careful, and their balanced presentation of detailed data makes the volume especially useful in understanding the changes and tensions in contemporary China.

Jurgen Domes's book is certainly the most valuable of the three volumes reviewed here for political scientists. Domes is a well-known scholar, indefatigable in his research, who works both in China and in Taiwan. This book focuses on the post-Mao reforms. There is some discussion of the Chinese political tradition and the geographical and economic constraints on China. Much the largest part of the book, however, is devoted to the issue of leadership transition and includes analyses of key individuals, the compromises of 1980–83, and the possible lines of future development. Domes's book is a gold mine of information on elite backgrounds and characteristics. More than most scholars, he has the knack of organizing his work so that it maintains its relevance in a constantly changing field. This is a very useful book for the scholar and the more advanced student.

JOYCE K. KALLGREN

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Davis

Sozialpartnerschaft in der Krise: Leistungen und Grenzen des Neokorporatismus in Österreich. Edited by Peter Gerlich, Edgar Grande, and Wolfgang C. Müller (Vienna: Böhlau, 1985. 412p. oS386, DM58, paper).

Austria's social partnership, an institutionalized system of cooperation of the main interests among themselves and with the state in the making and implementation of economic and social policies, has been identified with the country's economic successes of past years. How will new challenges, such as the economic crisis of the eighties, the need for industrial restructuring, and the emergence of post-materialistic movements affect this system? Will it adapt or break down? Will it contribute to or impede Austria's response to these challenges? These questions explain the title of the volume, which contains contributions by political scientists and economists (academicians as well as practitioners) from Austria and the Federal Republic of Germany. But the focus of the volume is broader than is indicated by the title. It deals not only with the crisis-related questions mentioned above. It discusses not only the Austrian social partnership, but also the broader concept of neocorporatism and the theoretical debate about it. Thus the structure of the book, apparent in the chapter titles (e.g., "Preconditions and Development of Social Partnership," "Social Partnership and Politics," "Social Partnership and the Economy"), obscures an underlying, different structure. Some of the 14 chapters are almost exclusively devoted to the Austrian case (chaps. 2, 3, and 9–12), while others emphasize to various degrees theoretical and comparative aspects (chaps. 1, and 4–8).

The chapters with a theoretical or comparative focus are *prima facie* of greater interest to readers whose interest extends beyond the Austrian case. Among them, the chapter "Social Partnership in Comparative Politics Research" (pp. 85–107) by Gerhard Lehmbruch together with Philippe Schmitter, one of the initiators of the discussion on neocorporatism, is a valuable contribution to that discussion, as are the chapters by the editors. The chapter by Wolfgang Blaas and Alois Guger, "Industrial Relations and Macro-Economic Stability in International Comparison" (pp. 255–77) draws on data from 17 OECD countries for its conclusion that macroeconomic stability is more likely in countries with a cooperative structure of industrial relations and a cooperative social climate than in countries with a conflict-oriented social climate.

The chapters focusing on Austria provide new and valuable insights, especially when they deal with hitherto somewhat neglected aspects of social partnership such as the chapter by Müller on the role of the political parties (pp. 135–224), the chapter by Grande on the relevance of legal norms for conflict management (pp. 225–54), or the chapter by Nowotny on the relationship between finance policy and the functioning of social partnership (pp. 313–29). These insights are useful in a broader sense because of the paradigmatic significance of this special case of neocorporatism.

There is no unanimity among the authors about the existence of a crisis of the social partnership nor as to whether there are grounds for optimism or pessimism. Günter Chaloupek and Johann Farnleitner, whose views represent those of two components of the social partnership—namely the Chamber of Labor and the Chamber of Trade respectively—are most optimistic (pp. 331–54). But the authors agree in general that the social partnership is more than a "fair-weather product," for instance Talos (p. 83), Grande (p. 244), and Farnleitner (p. 349). The more pessimistic authors develop scenarios regarding the possible effect of the economic crisis on the stability of the system. Under conditions of economic growth, corporatist cooperation is a non-zero sum game, in which all participants profit. When a decline in economic growth leads to a zero sum situation, will the unions under pressure from the rank and file give up their cooperation, or will capital do so in the face of a sharpening international competition? (see, e.g., pp. 17–18, 106–7). As for the unions, Grande (p. 245) and Blaas and Guger (p. 259) point out that the aggregation of rank and file demands into a "solidaristic wage policy" allows a monopolistic, centralized trade union federation to adjust wage demands to long-range considerations of the nexus between international competition, higher wages, and the employment situation. As for the employer side, Ewald Nowotny (p. 322) believes that the trend in other countries toward more aggressive strategies (e.g., use of the threat of increased unemployment to achieve wage restraint) will not be followed in Austria because of the strong position of the unions. Lehmbruch also notes (p. 91) that the use of unemployment as a resource for economic policy making remains a taboo. Chaloupek (p. 342) sees no evidence of a more aggressive incomes policy on the part of the unions.

Still, there is broad agreement that present tendencies will or should lead to adaptations in the social partnership. Wolfgang Fach and Gerd Gierszewski, in the chapter most critical of social partnership, foresee a transition from the present "gentle" corporatism to an "austere" corporatism in which the social partnership will allocate the cost of the economic crisis asymmetrically to the wage earners (pp. 291–94). Gerlich (pp. 1226–32, 366) and Nowotny (pp. 327–28) see a possible adaptation in a partial decoupling of the social partnership from government and a restriction of its activities to the sphere of income and employment policy with greater autonomy in that sphere. Müller (pp. 180–208) documents such a tendency by case studies.

There is also the question whether the social partnership contributes to or impedes Austria's response to the economic crisis. Is it flexible enough to make a contribution, or is it fostering stability while handicapping dynamism and innovation? In particular, does it handicap the process of industrial restructuring, which is necessary under present circumstances? Chaloupek, who deals with the question in some detail (pp. 336–40), does not share the "structural pessimism" of others, and he is satisfied with the tempo of structural adaptation. But other answers to these questions are ambivalent, indicating some doubt about the flexibility of the system (pp. 285–90, 363–64). Georg Winckler believes that the social partners will discover their long-range interest in restructuring and innovation once they recognize that there are budgetary limits to the assumption of risks by the state. The question is to what extent they will be able to bear the costs of cyclical and structural risks that threaten to disrupt the explicit and implied understandings between them (p. 312).

This sketchy tracing of some of the themes of the book may have indicated its richness in details and insights. Contrary to many edited volumes, the contributions are of almost uniformly high quality. The extensive bibliography (pp. 367–95) is an additional bonus.

KURT STEINER

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The Political Economy of Corporatism. Edited by Wyn Grant (New York: St. Mary's Press, 1985. xii, 274p. $29.95).

Varieties of Corporatism: Theory and Practice. By Peter J. Williamson (New York: Cambridge University Press, 1985. x, 244p. $39.50).

Far from assuming the status of a normal science, research in political science is increasingly informed by distinctive and competing paradigms of political and social change. To pluralism and Marxism (in their many varieties) must be added public choice analysis and corporatism. While corporatist thinking is hardly new—it is now more than fifty years since Mihail Maniolesco published his *Le Siecle* *du Corporatisme*—it has received fresh impetus over the last 15 years as a model applicable to politico-economic arrangements in advanced capitalist countries.

As theory, however, corporatism has not advanced very far. Unlike pluralism it has failed to generate a rich and complex body of empirical research. And it patently lacks the intellectual and emotional appeal of Marxism or the clarity and analytical power of public choice analysis. Both of these volumes are aware of these failings and, indeed, are attempts to provide greater theoretical coherence to corporatist thinking.

The Political Economy of Corporatism is an edited volume in the St. Martin's series, *Sociology, Politics, and Cities.* Perhaps this explains the slightly odd juxtaposition of four chapters on the relationship between corporatism and local government and politics (mainly concerning Britain) and five rather disparate chapters on corporatist theory, Austria, industrial relations, and the European Community. Wyn Grant's introduction does provide some unity to the volume by stressing that all the contributions concern "societal" as opposed to "state" corporatism. Societal corporatism is "characterized by the existence of singular, noncompetitive, hierarchically ordered representative organizations which are autonomous in their origins. Through an osmotic process they develop a symbiotic relationship with the state, so that the legitimacy of the state becomes in part reliant on the active consent of recognised interest organisations" (p. 10). Grant offers further refinements on this theme, but unfortunately none help very much. Societal corporatism ends up as a catch-all description on the complex pattern of state-societal linkages that prevail in all western liberal democracies.

Philippe Schmitter's contribution labels the same phenomenon "neo-corporatism." Schmitter is eager to disabuse his audience of any inference that neocorporatism is behavioral (i.e., depends on the preferences of civil servants) or functional (i.e., serves the interests of capital) or tactical (i.e., gives the impression of state neutrality while really serving some dominant class). Instead, neocorporatism is *structural,* or "grounded in the institutional interests of the state" (p. 41). But what are these interests? In effect, they consist of the constitutional and institutional status quo. In stable democracies, politicians and civil servants want to remain in power, but there are structural constraints on the things they can do to remain so. It is these constraints that help mold state-societal relations. The obvious retort to this theoretical refinement is, so what? For, analytically, it adds little to the noncorporatist perspectives of the pluralists and neopluralists (Almond, Lindblom) or such non-Marxist theorists of the state as Nordlinger.

Of the remaining contributions, the most important is Bernd Marin's piece on Austria, in which he systematically demonstrates that in many crucial respects, Austria is not the paradigm case of what he calls liberal (or societal or neo-) corporatism.

Peter Williamson's book on *Varieties of Corporatism* is a serious attempt at systematic political theory. His task is a formidable one, for he sets out to test the analytic utility of the concept of corporatism. He does this by first examining what he labels *consensual-licensed* corporatism, or the essentially non*statist* corporatist thinking which emerged in Catholic states following the demise of the *ancien regime.* Second, he studies *authoritarian-licensed* corporatism, or the politico-economic systems associated with fascist Italy and Salazar's Portugal. Finally, he returns to postwar neocorporatism, which he duly relabels *contract*-corporatism.

Williamson writes well and thinks clearly. For each of his models, he sets out a list of defining characteristics preceded by a full discussion often studded with empirical references. His chapters on *consensual-licensed* corporatism primarily concern the communitarian thinking of such writers as Maniolesco, La Tour, and Othmar Spann. Unlike the authoritarian corporatists, they had little opportunity to put their theories into practice, so Williamson is concerned only with the coherence and unity of their ideas. His chapters on Italy and Portugal are insightful and useful. But when he tackles postwar corporatism in Europe he encounters the same problems as Grant and Schmitter: What precisely is the role of the state in corporatist theory? Can neocorporatism easily be distinguished from pluralism? Are the varieties of corporatism (within and between countries and over time) so vast as to invalidate the approach as a useful analytical tool of political science? Unfortunately, Williamson is unable to answer these questions. Like the Grant volume, his book, while interesting and sometimes provocative, adds as much confusion as clarity to the ongoing debate on corporatism. Ultimately, corporatist theorists depend on open-ended taxonomies rather than on a theoretically powerful central idea based on some conception of human nature or of economic interest. Along with other middle-range theories, corporatism is most valuable when we accept its limitations and use it as an adjunct to other more powerful theories of political and economic change.

DAVID MCKAY

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Spain After Franco: The Making of a Competitive Party System. By Richard Gunther, Giacomo Sani, and Goldie Shabad (Berkeley: University of California Press, 1986. xix, 516p. $42.00).

Certain political events are so dramatic and so emblematic of larger issues that they seem virtually to tell their own story. Getting the facts straight may be the paramount challenge when, as with revolution or civil war, access is difficult, institutions are breaking down, ideological distortions are rampant, and the language of the protagonists has to be learned from the outside. The temptation is to let well enough alone: to assume that, if these difficulties can be overcome, it is superfluous to spell out the theoretical and cross-national implications of the conflicts under study. This approach is attractive to the degree that comparative politics often provides little in the way of rigorous conceptualization of rapid change.

If such a narrative strategy is to work, it has to depend on the skills of the storyteller and on a fascination intrinsic to the events that make up the story. Richard Gunther, Giacomo Sani, and Goldie Shabad are knowledgeable observers of Spanish politics. However, they recognize that, unlike the civil war of the 1930s, the transition to democracy in Spain that got under way in the 1970s has been occasionally exciting but mostly a matter of prolonged interelite bargaining and of ambiguous shifts in public opinion. At the same time, they also understand that the success of the Spanish transition was scarcely foreordained. Although there has not been much political theater in contemporary Spain in the histrionic sense, drama of another kind has been plentiful.

It is in working the area between calculated efforts at conciliation and the threat, never far from the surface, of explosive confrontation—as exemplified in the deals struck by party leaders on religious, regional, and economic controversies once thought to be intractable—that *Spain after Franco* makes a real contribution. The drama may be oblique, but it is present nonetheless. Interviews with over 100 national and regional elites, careful attention to institutional regulations—for example, changes in electoral laws—and a lucid reconstruction of events from an abundant primary literature (newspapers, magazines, party documents, and memoirs by some of major actors themselves) enhance our understanding of the early days, from around 1976 to 1982, of the new party system. This is no mean feat. Rather than indulging in yet another world-historical overview of democratization—a genre that has come to consist for the most part in bows toward Gramsci and studied neglect of Lipset—the authors deliver an important account of short-term, grounded learning. This is refreshing, and it makes for a good read.

Yet, in other respects *Spain after Franco* is a disappointment. Much of the empirical analysis, especially the several chapters devoted to a mass survey conducted in 1979 with a brief look at survey results from 1982, is weak. And the book's contribution to democratic theory is marginal.

Analytical problems are not confined to the surveys of public opinion. Invaluable as they are as oral history, the elite interviews are occasionally underinterpreted. The elites are treated as informants rather than respondents. They are sources of descriptive information. But their discourse itself, the rhetorical style of the elites—a tendency to express calls for moderation with a quasi-religious fervor, as well as other slips between form and content—is overlooked. Moreover, the substantive value of these interviews hinges on the frequency with which the elites reveal perspectives that go beyond common knowledge of the sort available in newspapers. A by-product of *Spain after Franco* is that it confirms the suspicion that some Spanish elites read the newspapers but have few novel insights. Finally, the authors do not give sufficient emphasis to the synergetic combination of generational resentment, grasp of the utility of the mass media, boredom with extremist posturing, and lower-middle-class ambition that gave inexperienced leaders who were widely thought to be mediocre and even authoritarian such persistence, and such an opportunity, to push for democracy. Without taking these factors explicitly into consideration, it is difficult to understand why men like Adolfo Suárez, Felipe González, and others displayed such a mix of moderation and determination and succeeded when the obstacles to success were so high.

Still, these blemishes are minor in comparison with the problems encountered in the handling of the mass survey. The analysis of the data is organized around class, religion, regionalism, and left-right orientations. The approach is sensible insofar as these are the major cleavages of Spanish politics. But the outcome is a string of findings that fail to hang together and that are on occasion of doubtful validity.

The problem stems from a lack of cross-national comparability in the items and, more broadly, of a theoretical architecture to the questionnaire. The static nature of the survey need not have been so restrictive had the authors included items that could have been matched with questions from surveys carried out in other industrial democracies or had they shown greater familiarity with phenomena addressed by the survey research community, for instance, economic voting, system support, protest activity, and the like. Perhaps the most damaging lapse, in a book about the development of partisanship and the party system, is the failure to use standard or even plausible indicators of partisan attachment. Another glitch is an indiscriminate reliance on feeling thermometers and other shopworn gadgetry of the survey method.

The initial justification for resorting to such a parochial version of survey research must have been the belief that Spanish politics is "unique." The fallacy is evident: there is no way to establish or reject this idea if some measures are not comparable in the first place. Besides, as the authors surely appreciate by now, the key question concerns the relatively distinctive features of Spanish politics, not its supposed uniqueness.

The analysis compounds errors built into the design of the survey. Not a single causal model is estimated in the more than 200 pages of *Spain after Franco* devoted to the statistical study of the mass survey. Instead, the presentation plods along with cross-tabulations, correlations, and factor analyses. These are computational preliminaries that might more suitably have been relegated to an appendix. The what-goes-with-what mode of exposition not only neglects multivariate techniques but it also confuses independent with dependent variables and sometimes generates quite improbable inferences, such as the notion that younger Spaniards are more likely to consider themselves of the working class than are older Spaniards.

While these mistakes reflect limited methodological refinement, the root of the problem is theoretical rather than technical. As an example, the claim that the 1982 election, in which the incumbent center-right party collapsed and the socialists were swept to power, constituted a realignment assumes not only that partisanship has been adequately measured but also that the Spanish party system was aligned, rather than inchoate, to begin with. The first assumption is false. The second, in a new party system, is tenuous.

On balance, *Spain after Franco* has many of the virtues and some of the defects of a pioneering work. It is an indispensable guide to the elite negotiations that set the Spanish parties off and competing under conditions of great uncertainty. In this regard, the study is as close to definitive as we are likely to get. It is a flawed analysis of public opinion. Its contribution to democratic theory and comparative politics is tangential. Like its subject, it is a transitional book.

PETER MCDONOUGH

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Politics in Britain and the United States: Comparative Perspectives. Edited by Richard Hodder-Williams and James Ceaser (Durham, NC: Duke University Press, 1986. xvi, 232p. $35.00).

A group of scholars from Britain and the United States gathered at Duke University in 1984 to commemorate the founding of the doomed colony on Roanoke Island 400 years previously by discussing developments in British and U.S. politics. The present volume contains the revised papers presented at the conference.

The participants in the conference were by and large well-known political scientists distinguished for their contributions to the study of either British or U.S. politics; only one of the participants has engaged in extensive original research on both countries. As one might expect from the participants in the conference, the essays are informed, interesting, and sometimes thought-provoking. Most suffer from the fact that their authors have much stronger credentials to write on one country (not necessarily the country of which they are a citizen) than on the other. The essays sometimes read, therefore, as though their authors had tried diligently to come to terms with secondary literature with which they were not previously familiar and that they would not have confronted had it not been for an invitation to this conference. One contributor more or less acknowledges this point by describing his essay as an elaborate reply to a final examination question that he might have been set. Heclo's essay stands out in terms of its quality, in large part, I suspect, precisely because he is the participant with the best claims to have conducted original research on both countries.

The scholars who assembled at Duke are at their strongest when they discuss the traditional core subjects of British and U.S. politics courses. The chapters on the legislature and executive, bureaucracy, judiciary, and party systems either summarize adequately existing writing on those subjects or provide—usually within the author's area of expertise—some illuminating insights. The least satisfactory section of the book attempts to explain the rise of laissez-faire economic thought in both countries without coming to terms satisfactorily with the enormous body of literature which exists on the political economy of the two countries.

Two more general reservations may be made about the volume. First, potential purchasers should be aware that two chapters—King's on scandals in Britain and the United States and Donoughue's on economic decision making during the last Labour government (apparently there was some) have been published elsewhere. Somewhat scandalously, Donoughue's piece appears in another collection from the same publishing house. Second, the participants in the conference, though rightly proud of continuing the long tradition of comparing British and U.S. politics, were almost willfully blind to the interesting comparisons that can be made between the United States and European political systems other than Britain. Heclo mentions only to dismiss from further consideration the fact the continental European bureaucracies often have features contrasting even more vividly with U.S. bureaucracy than those of Britain. King ends his essay on the study of scandals with a call for more research on the topic (though I suspect more may have been done already than he acknowledges), particularly on France. One of the many books by Philip Williams, to whose memory this book is dedicated, was partly on that topic. However, King's observation is surely sound in its implication that U.S. politics might fruitfully be compared not just with British but with other European political practices, too. This, too, is, on reflection, a point that Williams himself made many years ago.

GRAHAM WILSON

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Labour and the British State. By Barry Jones and Michael Keating (New York: Oxford University Press, 1985. vi, 214p. $29.95).

The basic argument of this book is that the Labour party has consistently failed to challenge the form and institutions of the British state and, instead, has adapted itself to them. The existing state is not a suitable instrument for the implementation of Labour's goals. Consequently, the party's intentions have been continually frustrated by its failure to confront the limitations of the existing state. To demonstrate this thesis Jones and Keating examine four areas: management of the economy; the handling of territorial issues; the machinery of government itself; and the issue of British sovereignty. In each area a case is made for the ambivalence and incoherence of Labour's perspective on the state. Lacking a critical perspective of its own, the party became attached to what was already there and, in recent times, has become identified as *the* party of the British state. Jones and Keating characterize the nature of this state as remote, alien, centralist, undemocratic, and bureaucratic. Writing from a fashionably decentralist perspective, they view all of these features as undesirable and contrary to the basic purposes of the Labour party.

How did this situation come about? Labour's ambivalence and lack of clarity about the state was the result of three factors: the continuity of British political institutions and the absence of a revolutionary democratization of them; the influence of the atheoretical British radical tradition, of which Labour is very much a part; and the inconsistent objectives of different components of the Labour coalition. Ultimately these factors produced an intellectual failure to develop "a more sophisticated understanding of the nature of the state and the political system" (p. 72).

The authors' definition of the state (p. 2) conjoins the notion of a political community existing on a specific territory with that of a complex of institutions operating within a given society. The territorial dimension (which provided their point of entry into research on the Labour party) has priority in their analysis. Generally, the sections dealing with territorial and national issues are the strongest in the book. Labour's centralism and incomprehension of the national question (most notably in Ireland) *has* made it difficult for the party to handle these issues.

What is much more problematic is whether Labour's acceptance of existing state (in its societal rather than its territorial dimension) has posed major difficulties for the party in its dealing with economic and other issues. Given the modest and limited nature of the changes they wished to introduce in British society, were Labour's leaders wrong to suppose that the existing state would serve their purposes? There is no particular reason to think so. Had Labour's project been more radical, its failure to develop a critical theory of the state would have been a greater liability, and Jones and Keating's argument on this score more convincing. In fact, their book is generally less convincing on the nature of the Labour party than the Marxian analyses they reject (pp. 67–71). Certainly the consistency of Labour's practice, which Jones and Keating recognize, can be better understood as a logical product of social democratic ideology dominant within the party, rather than as a product of its failure to comprehend the nature of the state.

Overall, however, *Labour and the British State* is a useful addition to the literature and deserves to provoke considerable discussion.

STEPHEN MCBRIDE

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Marxism and the French Left: Studies in Labour and Politics in France, 1830–1981. By Tony Judt (Oxford: Oxford University Press, 1986. ix, 338p. $29.95).

It is hard to dislike a book that makes such deliciously nasty remarks about French Marxist intellectuals and their cross-Channel and transatlantic admirers. How account for the bloated reputations abroad of a succession of French intellectuals from Jean-Paul Sartre to Louis Althusser (and many lesser lights between) engaged in Marxist discourse? What was political in Paris, Tony Judt points out in a thoroughly enjoyable chapter, "French Marxism in 1945–1975," became theoretical in London and "merely academic in its final resting place further afield"—for example, in the *New Left Review* and the Yale English department. "Unable to grasp the rather particular circumstances of intellectual production in France, scholars and acolytes abroad could only attribute the intensity of feeling aroused by the works of Parisian *maîtres à penser* to an originality and power of pure thought, to the identifying of which they have since devoted thousands of pages" (p. 169).

Tony Judt, fellow and tutor in politics at St. Anne's College, Oxford, puts that intellectual production in proper perspective. The writings of French Marxist intellectuals that appear from abroad as disembodied verities, he argues persuasively, can be understood only as part of an ongoing debate *within* the French Communist party about its role in the polity. For Parisian intellectuals, history made it impossible to be against the party; yet they were also repelled by much within the party. They had to work out their own position of support from the outside—and some, from the inside. What foreigners thought of as self-absorbed and timeless existentialism, comments Judt, "was to its founders and their colleagues an endless search for the philosopher's stone of a phenomenological marxism that could also *act*" (p. 187).

Sartre transcended the difficulties of the moment—the unappealing features of the real French Communist Party (PCF)—by placing faith in the cunning of history and structures. His point was grasped and further developed by Louis Althusser—who remained at all times a party member. Considering the shallowness and duplicity of party leaders Maurice Thorez, Jacques Duclos, and the egregious Georges Marchais, it made sense to abstract the role of human agents from a history determined by the interplay of impersonal structures and practices. Judt's critique of Sartre and Althusser is highly recommended for anyone who still entertains the idea that they were independent spirits.

The chapter on French Marxism is one of four long essays, plus an introduction, on disparate topics: the French labour movement in the nineteenth century; the French Socialist party from 1920 to 1936; and the elections of 1981. Each essay is self-contained and thoughtful, but it requires an effort by the reader to discern any unifying theme. Judt argues that Marxism is now history in France, and that French communism is on the way out as a powerful and autonomous force. He believes that the 1981 election was a decisive turning point in the evolution of the party system, marking an irrevocable break with revolutionary posturing and illusions by the Socialists.

How could the venerable rivalry between reformists and revolutionaries be so easily transcended? Judt contends that the historical divisions within the French Left have been exaggerated. Thus, the entire French working class was receptive to Marxist notions even before Marx appeared on the scene, and was oriented towards revolutionary activity by the repressive policies of Louis Bonaparte. For Judt, Proudhon's anarchism was always a minor trend, overshadowed by faith in the state as an instrument of working class aspirations, and "wrongfooted" to boot. Communists and Socialists after 1920, he believes, shared a revolutionary orientation. The Left is now expressing its underlying unity through one party, rather than through two parties as in the past.

In my view Judt greatly underestimates the internal rift of the French Left, downplaying Proudhon and the significance of the competition between Jaurès and Guesde and now between Socialists and Communists. He is thus led to strange or misleading judgments. Michel Rocard, he states, "is isolated today in his political family" because he is outside the political culture of the mainstream Left. Considering that Rocard is a leading Socialist candidate for the presidency, this is a curious view of "isolation." Judt makes no mention of the official conversion of both Socialist and Communist parties to autogestion in the decade of the 1970s (an indication of the force of Proudhonian tradition). He characterizes the student leadership in the May 1968 revolt as consisting of Trotskyists and "non-ideological radicals"; to say that the anarchists and surrealists who made up one wing of the student movement had no ideology is simply wrong. Judt is also in error in describing the Common Program of 1972 as a platform for social democracy; that was certainly not the intention of the PCF. And he overlooks altogether the significance of the intense debate within the PCF over Stalinism and its causes and its determination to create a "democratic socialism," as contrasted with the "authoritarian socialism" that it now considers to exist in the Soviet Union. That the PCF has not been able to extricate itself from its contradictions is true, but its efforts to do so are worth recounting and explaining.

In 1981 the Left took power, and made the inevitable discovery that a belief in utopia does not resolve pressing political problems. Socialist intellectuals have become more aware of the needs of a complex society as they strive to advance the interests of workers and the disadvantaged. The Socialist party is now a credible party of government. Tony Judt's book, for all its flaws, helps us understand that momentous development.

BERNARD E. BROWN

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Corporatism and Change: Austria, Switzerland, and the Politics of Industry. By Peter J. Katzenstein (Ithaca, NY: Cornell University Press, 1984. 331p. $35.00).

The concept of corporatism—or "neocorporatism" or "democratic corporatism"—arouses surprisingly strong emotions among political scientists. Writers influenced by Marxism tend to view corporatism as a late capitalist mechanism intended to sublimate or suppress class conflict. For students of democratic theory, corporatism implies a secretive decision-making process carried out among economic and bureaucratic elites and largely excluding popular participation and sometimes even the participation of elected officials. Even among those scholars who have themselves contributed to the burgeoning literature on corporatism, commitment to widely diverging definitions of the term has often led to sharp disagreements.

Peter Katzenstein has invited the ire of all these audiences in his important comparative study of the political economies of Switzerland and Austria, and has compounded the risk by voicing nearly unabashed approval of the workings of what he terms "liberal corporatism" in Switzerland and "social corporatism" in Austria. He argues that, in both of these small Alpine states, corporatism succeeds in narrowing political inequalities through a process emphasizing consensus building and inclusion of the politically weak. The result is both economic success—even under the difficult economic conditions of the 1970s (the period covered by his study)—and an enviable level of political stability.

Katzenstein makes a persuasive case. He first presents the reader with concise but compelling overviews of the political economy of each country. He then compares the two directly and seeks to show that, in spite of their differences—the preeminent position of internationally oriented businesses and the particularly powerful role of the major private banks in Switzerland, the more activist role of parties and the state and the strong influence of the trade union movement in Austria—the two versions of corporatism bring about surprisingly similar results. These he attributes in part to their common vulnerability to pressures from the international economy.

Katzenstein undertakes to support these arguments with four case studies of industrial policy, examining the efforts of the two systems to assist and to manage the adaptation of industries especially severely affected by the rapidly changing economic conditions of the 1970s—textiles in both countries, steel in Austria, watches in Switzerland. Because the case studies focus on only one particular type of economic policy problem, they do not illustrate as broad a range of the different patterns of corporatist policy making in the two countries as one might wish. But they are carefully executed and illuminate much that is distinctive about the two systems in facing problems that have become familiar to virtually all Western states.

The critics, one suspects, will not be entirely mollified. Some will deny that Switzerland is corporatist at all (because of the weakness of Swiss labor and the lesser degree of institutionalization of corporatist structures—there is no Swiss equivalent, for example, of the Austrian Joint Commission for Wages and Prices). Others will argue that Katzenstein gives too little attention to underlying class conflict (as expressed, for example, in the growing ideological divide between the right wing of the Swiss *Freisinnige* (liberal) party and the left of the Social Democrats). To my mind, the most notable omission of the book is its failure to address except in passing the argument that the Swiss and Austrian systems, in their concern for accommodating major social forces and conciliating conflict before it becomes overt, frustrate democratic participation and marginalize political actors who fall outside the existing elite cartels. One might go so far as to argue that political conflict in Switzerland and Austria has been displaced from critical economic issues to less crucial, more emotional and symbolic arenas—youth rebellion and the Jura and foreign worker questions in Switzerland, atomic energy and leadership personality conflicts in Austria.

Nevertheless, Katzenstein had produced a finely wrought and challenging book. The argument itself is clearly and forcefully presented, and the body of evidence he has marshalled on its behalf is impressive. Not only the relatively small number of students of Swiss and Austrian politics will find this book invaluable, but also the much larger number concerned with the interplay between the international economy and domestic political institutions and policy processes. Implicitly, Katzenstein commends the neocorporatist policy-making approach to larger polities, none of which can claim any independence from the tides of the international economy and many of which have coped with them far less successfully than have Switzerland and Austria.

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Comparative Government and Politics: Essays in Honour of S. E. Finer. Edited by Dennis Kavanagh and Gillian Peele (Boulder, CO: Westview Press, 1984. viii, 242p. $32.50).

New Directions in Comparative Politics. Edited by Howard J. Wiarda (Boulder, CO: Westview Press, 1985. xiv, 239p. $35.00, cloth; $16.95, paper).

The comparative politics field is undergoing a difficult period of reflection; scholars are wondering what the configuration of the discipline really is, and they are debating about what direction holds the most promise for the future. Both of these volumes mirror the soul searching.

The Kavanagh and Peele volume is a collection of essays in honor of S. E. Finer, in which each contributor was encouraged to select a topic related to Finer's work. All of the writings deal with more than one polity (though some more extensively than others), and they focus primarily on substantive findings, with methodological issues receiving incidental treatment. The Wiarda book is an outgrowth of ongoing seminar sessions at Harvard, in which the participants took stock of the field and wrestled with questions of comparative methodology.

The work in tribute to Finer reveals the broad sweep of his intellectual interests. While a good portion of the analysis is centered on political institutions, the contributors also deal with political culture, national and local elites and their popular linkages, political participation, and electoral opinion.

The topics addressed by the authors are of current interest to political scientists. The strength of British government, according to Hugh Berrington, is really a source of weakness because it cannot claim that it lacks power to give interest groups what they demand; the governing party is reluctant to use sanctions against them out of fear of retaliation at the polls or through strikes. Vernon Bogdanor describes how the monarch is protected in the government formation process in five European countries. Jean Blondel treats the problem of "dual leadership," comparing the roles of the monarch or president and the prime minister, and the party secretary and the prime minister, and their impact upon regime stability. Raising some interesting research questions, Jack Hayward analyzes the socio-economic leverage exerted by pressure groups and the "social partners" strategy employed in France. Alan Angell traces the development and recent trends of military regimes in Latin America, detecting little uniformity (or effectiveness) in their economic policies. Donald MacRae outlines the evolution of elite studies, including their application to Marxist thought and communist thought and practice. In their chapter on popular participation, Geraint Parry and George Moyser wrestle with the definitional problem, and then draw attention to the importance of examining participation in community subcultures whose influence is lost in national samples. Research designs of linked case studies, they argue, are a prelude to theory development. In a comparison of constitutions, especially British and U.S., Gillian Peele indicates how legal norms reflect political culture and what factors are involved in the resolution of constitutional issues. In the concluding chapter, David Robertson attacks the Finer thesis that parliamentary representatives, initially harboring views divergent from those in the electorate, adopt policies that make concessions to party extremists, thus getting farther out of step with the more moderate opinions of the electorate. He contends that the measurement instruments are defective, and, drawing upon data from Britain, the United States, and New Zealand, he concludes that the voting population does not have a moderate position that is deserted by the parties.

The purpose of the Wiarda volume is to subject the comparative field to rigorous self-examination, with major emphasis on methodological approaches. Older ways of attacking problems, especially the work of the Social Science Research Council's Committee on Comparative Politics, are examined through a critical lens, and newer perspectives that are attracting support are brought into summary view, in terms both of their strengths and their weaknesses.

A listing of the essay topics will provide a glance at the divergent signposts that are being erected in the field: the struggle between the state and other social organizations (or even the market) over social control and the mobilization of mass support (Joel Migdal); corporatism (Douglas Chalmers); the "input" analysis characteristic of typologies of democratic systems from a liberal perspective and contrasting typologies based on political economy (Peter Lange and Hudson Meadwell); the dependency approach (Tony Smith); the rejection of Western models of development by Third World scholars, who are experimenting with alternative views of the process (Howard Wiarda); neo-Marxist emphases on class analysis (Ronald Chilcote); and comparative public policy (Lawrence Graham).

Nearly all of these essays are excellent descriptions of conceptual design and research trends. While indicating the potential utility of the various approaches, the authors do not make excessive claims. They are careful to point out deficiencies, and in some cases they present recent evidence that tends to weaken the paradigm they favor. Among the salient messages conveyed to the reader are the following: the state is a major factor in the political equation, becoming revived as an object of investigation; economic variables must be taken into account; indigenous institutions, which are more durable than earlier research designs assumed, sometimes play a useful role in the modernization process; and newly developed nations are not destined to follow the paths taken by the more advanced countries.

Striking criticism of past research efforts in the comparative vineyard, both explicitly and implicitly expressed in the two volumes, tempts one to wonder whether much has been accomplished during the last three decades. But Sidney Verba's chapter in the Wiarda book is a modulating influence. He puts the intellectual developments into a balanced perspective, his appraisal of the past includes a list of noteworthy accomplishments, and he attempts to bridge the old and the new, setting forth the needs and problems of the future.

The two books clearly illustrate how fragmented the comparative discipline has become. A host of flowers bloom, and divergent approaches contend. Intellectual ebullition, of course, can be a wholesome development, but it nevertheless signals the appearance of a critical point in the evolution of the field. (Other segments of political science similarly seem to be on a plateau, and self-criticism, with contending paradigms fighting for acceptance, will be helpful to these segments, too.)

Hans Daalder, in the Kavanagh and Peele book, displays an awareness of the crisis when he criticizes European scholars for not designing their studies within a comparative frame, and U.S. scholars for not being sensitive enough to contextual realities. Both criticisms need to be heeded; parochial studies by themselves contribute little to generalization, and failure to take situation and context into account results in faulty generalization. At a moment, the tendency in comparative research —reflected to some extent in both volumes—is to move away from the broader statements about the political world to more modest studies of phenomena in a given country or culture sphere.

This shift in research emphasis is understandable and can be worthwhile, but Verba's call for an amalgam of case studies and broader comparisons should be kept in mind. It is easy for scholars to become so preoccupied with parochial investigations that they lose sight of the broader challenge. Comparison, it is well to remember, is more a method than a field. All political scientists should be comparativists, for they are presumably interested in *commonalities*—statements about political phenomena that are applicable beyond the particularistic: across nations, culture spheres, and types of systems. The route to this objective, of course, is difficult to travel. But if scholars of politics fail to push ahead to wider generalizations, they run the risk of stumbling into a maze of "comparative uniqueness."

As many comparative scholars have pointed out, what is needed is a set of clearly delineated research problems that can be tackled from a variety of perspectives. The research designs must include concepts whose definitions have been thoroughly hammered out so that investigators will be studying the same thing, whatever their approach. In an earlier day, the concept *development* ran into difficulty, partly because it was defined in so many ways that researchers were studying different phenomena and the results could not be made cumulative. It will be a step forward if scholars working in particular countries were to address the same questions and, where possible, standardize their categorizations so that the findings will be additive and capable of being woven into broader generalizations.

A reading of these two books suggests that progress can be made in this direction; embedded in the essays are salient research problems that, with refinement, can be cast into comparative designs, and several of the approaches tend to converge in significant ways. Both volumes deserve the attention of comparativists who detect and are concerned about the crisis in their field.

JOHN E. TURNER

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East Germany and Detente: Building Authority after the Wall. A. James McAdams (Cambridge: Cambridge University Press, 1985. xi, 233p. $34.50).

This is a model monograph: well written, clearly organized, informative about important events, and analytical of political processes. Although narrowly focused, its findings are applicable to general concerns. One also has the pleasure of being stimulated by disagreement with some of its assertions.

McAdams deals with a small, relatively weak, and often derided state, the German Democratic Republic (GDR). Beginning with what is widely regarded as the nadir of the GDR regime's effectiveness, the building of the Berlin Wall, he shows how the successive GDR leaderships of Walter Ulbricht and Erich Honecker strove to buttress the regime's power with authority. They had to do this while coping with problems of economic and social development (similar to those of the other East European communist states). But above all, GDR leaders have faced a difficult, changing, and nearly overwhelming external environment. More particularly, the GDR has had to make its way between the often divergent interests of the more powerful Soviet Union and Federal Republic of Germany (FRG).

Over the perspective of the quarter century since the Wall, the current status of the GDR on the European and world stages, as well as among its own population, represents a considerable success story. McAdams not only tells this story well, he explores the sources of this success and proposes reasons for it.

Between a useful introduction and a rather cursory conclusion, McAdams develops a chronological and analytical account, beginning with the crisis of the Wall. There follow chapters on the GDR's post-Wall achievements, the erosion of that synthesis in the face of international change in 1967–71, the response of the new Honecker leadership to detente in 1972–78, and the GDR's management of stresses induced by the decline of detente.

Each chapter has some insightful observations on the interaction between the leadership's initiatives and the constraints on its actions. For example, McAdams places the Wall in the context of other, preferred solutions (West Berlin as a "demilitarized free city"), shows the role of economic reforms as an authority-building symbol (a point made earlier by Thomas Baylis), demonstrates how the GDR's "shell of non-relations" with Bonn collapsed after 1967 and what that meant to Soviet-GDR relations, depicts (and almost does justice to the personal drama of) Ulbricht's downfall in 1971, and shows, finally, how the current regime uses detente with Bonn to play an international role highly congruent with domestic authority building.

McAdams differentiates between tracing GDR policy back to a "legitimacy deficit," and his preferred formulation of "building authority" (see pp. 3–5). Since he does not define "authority," and authority building is forever threatened by West German flexibility, this is an uncertain distinction. McAdams makes abundantly clear how "penetrated" the GDR is by almost every move by Bonn or Moscow. Rarely has the Middle European "correlation of forces" been such as to allow East Berlin substantial leeway. The postponement, in September 1984, of Honecker's projected visit to the FRG demonstrated this very well.

Given parallels between Ulbricht's policies in 1968–71 and Honecker's in 1983–85, McAdams might have probed more deeply into Honecker's relationship to the USSR. He is surely correct in asserting (p. 151) that the key to Honecker's policy is the recognition that "socialist inequality need not mean total dependence or, perhaps more importantly, weakness."

Most of this account focuses on the international setting in which the GDR leaders pursued their goals. I would agree that this was more important than domestic forces, but it would have strengthened this book if the author had made this point explicit and substantiated it. But this is one of the minor flaws in a very worthwhile scholarly work.

HENRY KRISCH

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Bureaucrats and Ministers in Contemporary Japanese Government. By Yung H. Park. (Berkeley: University of California Institute of East Asian Studies, 1986. xii, 192p. $15.00).

Who really rules? The politicians? The bureaucrats? In Japan, the answer was easy for the first decade or so after World War II: the bureaucrats. They had exercised political authority before World War II. Needing them to bring about their reforms, the U.S. Occupation officials had never challenged that authority. In 1955, the Liberal Democratic party (LDP) became the majority party and formed the government. It still is the majority party. It still forms the government. Not even the opposition parties suggest that it will not be the core of future governments. In the 30 years it has been in power, the making of policy—government policy—has left the ministries and come to reside in one of the LDP organs, the Policy Affairs Research Council (PARC), composed of a gaggle of divisions, special committees, and investigative commissions. Currently, the gaggle numbers 121 committees and 144 subcommittees. Yes, it's a mess.

Scholarly writing on Japanese politics has trailed but generally reflected this change. Tokyo University professor Tsuji Kiyoaki wrote the first major postwar study of the Japanese bureaucracy. He argued that not only the bureaucracy but the junior bureaucracy exercised real political power. His book, first published in 1969 and now in its ninth printing, is still influential. Kyoto University professor Muramatsu Michio has challenged him in another powerful book on the bureaucracy, published in 1981. His thesis was that the bureaucracy could not deliver social stability and economic prosperity; these required the legislators and the parties, with the "interest groups moving in the background."

University of California at Humboldt professor Yung H. Park, the author of the monograph under review, accepts the upgrading of the politicians. Concerned with how the party exercises its authority over the bureaucrats, Professor Park focuses on the role of the cabinet minister. In his introduction, he claims, "The cabinet minister [is] one of the most underrated institutions of the contemporary Japanese political system." And he concludes, "Any study of agency policy-making . . . that leaves out the role and place held by the agency minister or ministers will be unable to unravel the full complexities of policy-making" (p. 9).

Why has the cabinet minister been so underrated? The LDP is composed of factions. In making a cabinet, each prime minister talks of putting the right man in the right post, but each prime minister must first allocate the seats among the factions and appoint to the seats men recommended by the faction leaders. The life of a cabinet officer is short. In the 30 years of the LDP's existence, its 12 prime ministers have significantly changed their cabinets 37 times; the cabinet officer often has no time to do his thing. The making of law and policy grows increasingly complex. Each minister confronts highly skilled bureaucrats, who have clear, sometimes unshakable ideas about what is their ministry's mission and what they should be doing to fulfill that mission. Furthermore, upper bureaucrats shield lower bureaucrats, who, traditionally, have framed policy. The demands of legislature, the party, and the constituency leave a minister neither time nor energy to perform as minister effectively. These circumstances suggest that a Japanese minister is weak. Bureaucrats have primacy. This view was broadly accepted when Professor Park started to write his book.

Professor Park argues that the semipermanency of the LDP has changed these circumstances. Bureaucrats cannot remain indifferent to politics and hope to advance in their careers. The higher ranking the bureaucrat, the more partisan he becomes in his outlook. In recent years, many lower-ranking bureaucrats have left their ministries to pursue elective office. Their frequent success has politicized their former colleagues. Before becoming a minister, an LDP politician will have served with distinction for at least 12 years in posts in the party, the government, and the Diet (national assembly). Though perhaps new to the problem of the ministry, the minister is a good politician.

Chances are, though, that a minister will have served on one of the PARC committees covering a ministry before becoming that ministry's minister. Even if he has not, he will have recourse to men in the party who are expert on that ministry's business. In fact, representing the party to the ministry and the ministry to the party will be one of the minister's principal tasks. Dealing with the special interest groups is another leg of ministerial responsibility and a source of ministerial strength. They will keep the minister informed; they will make demands. Tempering those demands will stand the minister in good stead among the officials. Satisfying those demands will stand the minister in good stead within the party. A clever minister will find a way both to temper and to satisfy. Nor will bureaucrats try to take advantage of a minister's short tenure in office because they know he will not go away when he steps down. He will take up occupancy in the PARC committee and they will have to deal with him there.

What have these developments meant to the organizational culture? They "have had a dampening effect on the career officials' sense of policy-making efficacy, making them noticeably deferential. . ." (pp. 179–80). In Professor Park's view, the politicians, not the bureaucrats, are on top.

That the bureaucrats and the politicians are contending elites in the making of policy is not a thesis unique to Japan. That this thesis represents the best way to analyze Japan has been questioned by Tokyo University professor Sato Seizaburo and LDP researcher Matsuzaki Tetsuhisa, now aide to the party president. In a recently published book, *Jimento seiken* (The Ruling Liberal Democratic Party) (Tokyo; Choukoronsha, 1986), a stunning display of political insight, they warn, "the ministries become weak to the extent that the Liberal Democratic Party becomes strong—that doesn't necessarily follow. We think it's a mistake to regard their relations as a zero-sum game" (pp. 78–79). Policy must be typed. The ministries will be stronger in reaching technical decisions. The party will be stronger in reaching political decisions. A major change between now and when the LDP was conceived is the dependence of the bureaucrats and the LDP politicians on each other.

Professor Park has worked long and hard on this monograph. In his preface, he says that between 1978 and 1982 he conducted over 200 interviews with persons "involved in and well informed about" Japanese politics. It took him four more years to digest what he heard but the resulting manuscript is a powerful statement, sophisticated and measured in its judgments not only about the ministers and bureaucrats but about Japanese decision making. Students of comparative politics will want to read the book because Professor Park has written it so that international comparisons can be made. Students of Japanese politics must read his monograph. Professor Park has erected the mile post from which future research will be measured.

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La complessità della politics. By Gianfranco Pasquino (Rome and Bari: Giuseppe Laterza & Figli, 1985. xii, 228p. 25,000 Lire, paper).

This volume consists of seven essays, all previously published by Pasquino between 1983 and 1985 as journal articles, chapters in volumes, or independent monographs. The topics range widely, but the individual pieces constantly come back to the same themes. These themes are rather more broad than the title of the book actually suggests; at the very least, they carry the notion of "political complexity" to some of its logical conclusions. The major issues that, in one guise or another, are put before readers are the crisis of the welfare state (broadly defined), the adequacy of modern political institutions for coping with the problems of the late twentieth century, and, perhaps above all, the ability of the mass parties of the left to continue to perform a progressive, reformist function.

Pasquino's approach is comparative, and several essays could serve as models of how a review of literature ought to be carried out. The discussion is almost always very sharply focused on a single problem as the author explores how well various theoretical approaches (ranging from systems theory through neocorporatism and some of the most common applications of political economy) clarify the themes that have been at the center of recent debates in Italy and elsewhere.

But while Pasquino covers a number of comparative cases, it is clear that his ultimate concern is with the reformability and adaptability of *Italian* political institutions and parties. He certainly does not understate the problems that have raisen since the late 1960s. Indeed, his syntheses of the difficulties facing modern democratic systems (chap. 6) and reform parties (chap. 3) are some of the most lucid I have read anywhere. But the author very strongly feels, in contrast to many of the analysts he so ably summarizes, that both politics and parties not only still matter but represent the only hope for successful resolutions of present difficulties within a genuinely democratic framework. At the same time, he acknowledges that the changes required of existing institutions will have to be extensive if there is to be any hope at all of successful reform. Unpersuaded by the conservative bias in many contemporary approaches, he nevertheless underscores the important insights of these approaches. Readers familiar with his numerous contributions to Italian political science will in fact find that these essays, taken together, not only have their own internal unity, but also illuminate why in recent years this astonishingly prolific author has felt it important to address a great number of issues in several important volumes and countless articles (on Italy's three major parties; on terrorism; on the media and politics; on voting behavior; on institutional reform; on social complexity and new social movements; and on Italy as a political system).

The book itself consists of three sections. The first two chapters primarily explore different approaches to crisis and systemic complexity within the literature of political science, both mainstream and Marxist. Here, Pasquino is mainly concerned with showing both continuity and evolution in the literature on modernization and political complexity. The next two chapters focus on the problems of a reform party and on mass media and communications, but the latter chapter keeps coming back to the impact of new modes of communication on mass parties. The final section contains three chapters that address various aspects of political systems and policies as they confront the challenges of governability and representativeness in the 1970s and 1980s. Chapters 6 and 7 also contain extended expositions and critiques of the literature, ranging across U.S. and European contributions. Pasquino is most genuinely comparative in this section, drawing primarily on the U.S., Sweden, and Italy as cases of different states and societies confronting similar problems.

The volume suffers from the shortcomings of a collection of essays produced for a variety of other sources. Ironically, the strength of the review of the literature in some of the individual essays erodes the impact of the book as a whole. The very succinct treatment of a single author or body of work, or the abrupt shift from spelling out a problem to discussing how it is viewed in the literature, is far more suited to an article than to a longer treatment. When the reader encounters the same author, or title, on a variety of occasions in the course of the collection, he begins to wish for a bit more depth (or for a more integrated treatment). Such repetition is by no means a crippling flaw, and it does serve to underscore what the author considers most important. Yet it does weaken the impact of this particular book. Those who want more depth from Pasquino on many of the specific issues raised here will have to refer to his abundant other works. Still, the ability to sort out and make clear the central propositions concerning social complexity, fragmentation, and "governability" found in a variety of seemingly unrelated writings and writers is, as already noted, a model of synthesis. And Pasquino's discussion of both the past performance and problematic future of mass parties is as solid and insightful as one is likely to find anywhere, in any language.

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Public Employment in Western Nations. Edited by Richard Rose (Cambridge: Cambridge University Press, 1985. xi, 265p. $39.50).

This volume is one of several that have recently been produced by the Center for the Study of Public Policy at the University of Strathclyde from its ongoing research into the growth of government. Its purpose is to document the extent of public sector employment in Britain, France, West Germany, Italy, and the United States since 1950. Therefore, the core of the volume consists of 100 tables that trace the growth, nature, pay, qualifications, and functions of public employees as well as the number of people primarily dependent on government incomes in each of these nations. In large measure, the text is simply a running commentary on these tables.

In statistical terms, the book is a prodigious accomplishment. The data have been gathered carefully so as to be comparable across nations. Country specialists have canvassed a wide variety of sources for each chapter. The material is well presented, and this volume is bound to become an indispensable reference work for anyone interested in the development of the public sector. In this regard, the Italian chapter is especially impressive. Moreover, there are some startling pieces of information to be found here: in virtually every European nation, the primary income of over half the population comes from the government, and since 1951 rising public employment has accounted for almost all the net growth in employment on that continent. These facts alone are sufficient testimony to the importance of this volume.

The analytic contribution of the volume, however, rests on the categories into which the authors have decided to group public employees and their products. In line with what they term a "programme approach" to public employment, Richard Rose and his collaborators divide the activities of public employees according to whether they contribute to social programs, economic programs, or the defining concerns of government (primarily tax collection, defense, and policing or judicial functions). Their results are then said to be either "collective goods," "marketed goods," or potentially marketable goods that are "given away." Perhaps unintentionally, the effect of this system of categorization is to impress on us the extent to which contemporary governments have moved away from their "defining" functions by virtue of a great increase in the number of potentially marketable goods that are "given away." A large increase in the number of educators and health care providers over the past three decades accounts for much of this trend because they are defined as public employees in almost all of these nations.

The relative increase in health and educational personnel is a striking finding and these categories have much to be said for them, but a few quibbles might be in order. Should doctors and other health workers paid indirectly through health insurance schemes of the sort found in France and Germany, for instance, be labelled "public employees"? Most of the funds that pay them are channelled through the state, but I suspect that many European physicians might be surprised to see themselves described this way. In this volume the term *public employee* includes people who stand in such a wide variety of relationships to the central state that it loses some of its meaning along the way Similarly, there are those who might be inclined to consider education to be as much a "defining concern" of the modern state as defense; and there are those who (stressing its social benefits) might see education as a collective good rather than as a potentially marketable good now given away. After all, as some U.S. communities have demonstrated, even many policing functions can effectively be marketed. In short, there is room to argue with some of the broad categories into which this volume divides public programs.

Nevertheless, country specialists as well as comparativists will find a wealth of information in the chapters on Britain (by Richard Parry), France (by Edward Page), Germany (by Klaus-Dieter Schmidt and Richard Rose), Italy (by Andrea Cendali Pignatelli), and Sweden and the United States (by B. Guy Peters) as well as stimulation in the thought-provoking introduction by Richard Rose. This is an especially powerful salvo in the intensifying debate on the growth of contemporary governments.

PETER A. HALL

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Weapons of the Weak: Everyday Forms of Peasant Resistance. By James C. Scott (New Haven and London: Yale University Press, 1985. xv, 389p. $30.00).

The Muda River valley in the northeastern state of Kedah is the largest rice-producing area in Malaysia. In the mid-1960s, the government built a large irrigation system that permitted double cropping. Combined with the new green revolution technologies, this project more than doubled rice yields within a few years. While most of the farmers were small owner-operators, a significant minority were tenants or landless workers and a few owned substantial holdings. In this familiar Asian scenario, the latter constituted a local power elite because of their wealth, their control of employment, and their links with government and the dominant Malay political party.

At first the tenants and landless workers shared in the green revolution bonanza because double cropping expanded the demand for labor. But in a few years the larger landowners introduced combine harvesters and new methods of seeding, both of which substantially reduced labor requirements with devastating effects on the livelihoods of tenants and landless workers. The specific context of Scott's study is how the marginalized peasants responded to the imposition of new technologies and the commercialization of the rural economy, which threatened both their livelihoods and their status as productive members of the village community. The larger issue is a replay of the dilemma that has confounded Marxist social scientists—Scott employs the main class-based concepts of academic Marxism—why the victims of exploitation so seldom develop a revolutionary class consciousness and why they so seldom undertake revolutionary activity.

To examine these problems from the peasants' perspective, Scott employed the methods of the participant observer. Living and working for two years in a Malay village, he faithfully recorded daily interviews and encounters with members of all classes, kinship groups, and political factions in this village society. The result is a body of authentic data about peasant responses to material opportunities, threats, and deprivations, as well as the economic and social strategies they employed to defend their interests and rationalize their behavior—stripped of the deductive inferences so common in academic treatment of the world view and motivations of disadvantaged peoples. It is not only the sensitive reporting of this rich array of data but also their sophisticated interpretation by a scholar who has mastered the literature on peasant protest and resistance that makes this book a remarkable intellectual achievement.

Among Scott's major findings are these six: First, a continuous class struggle between owners of the means of production and those who provide only labor is waged over relative contributions to and shares in the production process, including wages, terms of tenancy, and even charitable obligations.

Second, this everyday struggle occurs simultaneously at two levels: the economic, in which the terms of material exchanges are worked out and the symbolic or ideological, where members of each class attempt to rationalize and legitimate their particular interests in terms of the social and religious norms that bind the community.

Third, while superior economic and political power confer overwhelming advantages on those who own land and capital, tenants and workers employ unremittingly the "weapons of the weak" to maximize their limited resources and bargaining power. These weapons range from slowdowns and informal labor boycotts to pilfering of rice and animals, careless threshing (so that more rice will be available for gleaners), and maligning by gossip the reputation of stingy and callous landlords. Occasionally, the resistance spills over to acts of violence, including the sabotage of machinery. With few exceptions, however, these tactics are carried out without formal organization and they avoid direct confrontation; indeed, on the surface they remain polite and deferential.

Fourth, contrary to the views of Gramsci and his numerous academic disciples, the rural poor do not suffer from "false consciousness" and are not "mystified" by the hegemonic ideologies promoted by the dominant class and its intellectual and political agents. The peasants are perfectly aware of what is happening to them, but they also recognize the limits of their ability to resist openly without jeopardizing the precarious livelihoods that are available to them and without inviting the repressive powers of local elites and the state. These peasants behave quite rationally in terms of their limited political, economic, and symbolic resources and opportunities.

Fifth, though sanctioned by religion and custom—in this case the Malay version of Islam—the solidarities of village life expressed in patron-client networks and in the obligations of the rich to ensure the subsistence of the poor were products primarily of economic interdependency. Landed farmers needed an assured supply of reliable tenants and workers. The new technologies substantially reduce the demand for labor. As the poor become redundant and expendable in the processes of rural production, the moral economy of village life changes. Landowners abandon—and find ways to rationalize the abandonment of—their traditional obligations to tenants and workers.

Sixth, the state increasingly penetrates the rural economy, but the services it provides disproportionately benefit the rural elites directly and by preemption. Moreover, its instruments of repression unfailingly protect the status quo and deter organized protest by the disadvantaged. In Malaysia the state has become the main provider of valuable public goods but has not succeeded in replacing the rural elites as patron at the village level. Many of the material benefits distributed by the state are mediated by the official party machine, which is controlled locally by landowning families.

Like most contemporary academic Marxists, Scott is explicitly pessimistic about the future of the marginalized peasantry under collectivized as well as capitalist regimes. In states that call themselves socialist, the peasants remain exploited and powerless. The class struggle over the remuneration of labor and shares of the harvest continues. Peasants resist the exactions of the socialist state by many of the same weapons that Scott identifies among their counterparts in the Muda River valley. In capitalist economies such as Malaysia, technology in the hands of a landowning class and their new commercial allies continues relentlessly to displace and marginalize tenants and laborers. At the village level they are fighting rear-guard actions with the ineffectual weapons of the weak while the growing power of the state inhibits and where necessary crushes any revolutionary responses. Contemporary academic Marxism provides increasingly sophisticated critiques of the "contradictions" of advanced capitalism but few plausible prescriptions on how to emerge from these dilemmas. The prevailing pessimism of the non-Leninist Left, as exemplified by Scott, offers even less hope for the victims of unrestrained capitalist expansion than the reformist formulas and the standard prescriptions for capitalist growth that Marxists continue to denigrate. Ironically, academic Marxism has achieved the peak of its influence among social scientists at the very time that its confidence in revolutionary action and in practical socialist alternatives has virtually collapsed.

The great strength of this study is its rich village-centered perspective on the unequal struggle over shares, material and social, in the wake of capitalist development. Its treatment of the state and its impact on local society, however, is fragmentary and incomplete. Though committed to capitalist development, the Malaysian state has implemented a growth-oriented economic strategy from which the majority of Malays, including many from modest village backgrounds, have benefitted in a relatively short period of time. It has retained enough democratic practices that it must be sensitive to its Malay constituency on whose votes the present ruling coalition depends (there is an active populist Malay opposition party whose role in Kedah's village life is described at some length in this study). To legitimate its rule, the present regime has taken a number of steps to improve the quality of rural life. These include the rapid expansion of such public goods as educational and health facilities, water supply and electricity. Rapid economic growth has provided alternative opportunities for livelihoods outside agriculture. The regime has guaranteed the political and cultural hegemony of the Malays, a not inconsiderable achievement of which village Malays are clearly aware. Though its machine-style rule is often heavy-handed and though benefits have been distributed unequally along class lines, the majority of rural Malays, and not only the most affluent among them, have benefitted, as Scott's data indicate. This is not to deny that the state could and should have done more—as reform-minded observers have urged—to mitigate the pace and the social costs of this rapid structural transformation.

The ability of a relatively successful capitalist state, through economic growth and the provision of public services, to distribute benefits and the prospects of benefits rather widely may help to explain the stamina of this pattern of political economy even in the face of destabilizing technological changes that produce a substantial number of losers and victims. Apart from its repressive apparatus, the relative stability of the Malaysian capitalist state to date can be explained at least in part by the material and symbolic benefits that it has generated for the majority of its core Malay constituents. This is the dimension of empirical reality and especially of theory that Scott underplays in this otherwise excellent study.

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The European Experience: A Historical Critique of Development Theory. By Dieter Senghaas (Dover, NH: Berg Publishers, 1985. 280p. $26.95, cloth; $10.50, paper).

This book describes the development experience of smaller European countries to see what light they throw on development policy throughout the world today. Countries are divided between those that are "autocentric" and those that are "peripheralized," following the Braudel-Wallerstein vocabulary, but blaming "peripheralization" on the policies of the countries affected rather than on hegemonic countries at the center. Small countries that are "associative" in the sense of producing raw materials and food for the world market are likely to find themselves in an economic backwater, although England, Belgium, Switzerland, and the Netherlands developed in that fashion, and an associative posture is satisfactory for large states. "Disassociative" development based on the dynamics of the domestic market has worked for France, Germany, and Austria-Hungary, for such state-capitalist economies as czarist Russia and Japan, and state-socialist economies as the Soviet Union, China, and North Korea. Socialist countries face the same choice as others as to whether to associate with or dissociate from other socialist countries.

What Senghaas holds to be the desirable model for developing countries is a combination of association and disassociation—export-led growth in specialized niches while keeping certain domestic markets protected, as exemplified in his judgement by Denmark, Sweden, Norway, Finland, Australia, Canada, and New Zealand. He regards free trade as a special case and views his position as neo-Listian, derived from Friederich List's prescription for a national economy.

Senghaas's recommendations go beyond association and disassociation with the world economy. He favors especially improving productivity in agriculture through defeudalization before setting out to build industry, wants a sharing of income and wealth as a basis for ensuring that increases in productivity spill over into consumption of domestically produced goods for the masses, not just conspicuous consumption that turns to luxury imports. He calls on developing countries to invest and innovate their own technologies and to build broadly effective infrastructures. The developing countries should stay self-reliant and decoupled as far as the Organization for Economic Cooperation and Development (OECD) world is concerned but associate collectively among their kind within regions and subregionally. These precepts, it is noted, do not apply to the successful newly industrializing countries (NICs) of the Pacific basin, but Senghaas is not discussing that experience.

The heart of the book is a historical account of the development experience of Scandinavia and the dominions (Australia, Canada, and New Zealand), the last presumably honorary parts of Europe. Denmark is compared with Uruguay on the (dubious) assumption that the resources of the two countries are similar, and some attention is given to the familiar Argentine-Australian comparison. On the other hand, too little attention is paid to France and Germany in Europe, the experience of which runs counter to his generalizations about an agricultural revolution and redistribution of income and wealth, or to the "semi-peripheries"—Italy and Spain. The European country that gets the most attention is Finland, stimulated in growth through paying reparations in industrial goods to the Soviet Union. Senghaas argues that the case is not exceptional but representative. The Norwegian legislation of 1905–10 limiting foreign ownership of natural resources is given critical significance as a disassociative act. Disassociative measures are called "clever" (p. 61) and "well-planned" (p. 100), with, however, little in the way of supportive evidence. I am inclined to argue that Scandinavia is not representative of European experience, with its initially weak feudalism and widespread Protestant education, plus the adventitious British repeal of the Corn Laws, the timber duties, and the navigation acts. While the Sandberg thesis of Sweden as an "impoverished sophisticate" is mentioned, no attention is paid to the financial institutions that constituted the core of Sandberg's thesis.

To those immersed in the Anglo-Saxon literature, the book is of great interest in calling attention in the footnotes to a lively and abundant literature on problems of economic growth, especially by Ulrich Menzel, who has worked with Senghaas as well as on his own, by Andreas Buro, Silvio Borner, the Swiss economist, and others.

The book, however, is hard going. Either Senghaas or his translator has a weakness for making up words. *Autocentric* and *peripheralization* are bad enough. There are, in addition: *dynamification, horizontalization, hierarchization* (and *de-hierarchization*), *hypertrophically*, and the like. Mouthfilling sentences of German structure abound. There are many meaningless but good words: *coherent, intermeshed, homogeneous, well-proportioned, balanced, broadly effective*, and the like, as well as their bad opposites: *heterogenous, disproportionate, stagnative, stunted, top-heavy, crippled, lame.* Italics protrude from practically every page to give a breathless quality to the prose, and the number of exclamation points exceeds the narrow limits I allow. Repetitive lists of conditions necessary for growth lead one to infer that economic growth is overdetermined with more equations than there are unknowns. The author uses *free-trade imperialism* and *staple theory* in ways that differ from normal practice among economic historians. There is no index.

Having said so much, I must admit that the book, while incoherent and badly proportioned, has some interesting ideas and refers to some interesting literature. It is especially refreshing that developing countries are not asked to metamorphize themselves into little Japans.

CHARLES P. KINDLEBERGER

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Three Civilizations, Two Cultures, One State: Canada's Political Traditions. By Douglas V. Verney (Durham: Duke University Press, 1986. 454p. $35.00).

This is a fascinating study of a number of what are clearly the most significant issues in Canadian politics in modern times. Verney investigates the manner in which Canada's current political culture and contemporary political institutions have been influenced by three different civilizations and seeks to explain not only how a number of uniquely "Canadian" practices and institutions have developed but as well what the implications of these practices and institutions will be for the future of Canadian politics.

Verney begins by drawing a rather unusual and important distinction between civilization and culture. Canada and the United States share a North American civilization; they do not share a culture. Canada has been "in the front line in the conflict between civilization and culture," he asserts, and each of three successive civilizations—the French, the British, and the U.S.—has "left a different residue" (p. 11) that has contributed to Canada's culture. Verney demonstrates how Canada has retained some cultural aspects of each civilization it has experienced, but yet avoided being absorbed by each of these cultures, seeking instead to develop its own culture.

There is a great deal of material here. Verney examines Canada's "philosophical federalism," and shows how Canadians were both drawn toward and fought against U.S. "scientific empiricism." He traces the manner in which Canadians, while attracted to British "Westminster" political institutions, "reconciled" (modified) these political institutions and practices such as parliamentary supremacy so that they would work within the Canadian federal political arena. He discusses the differences between Canadian and U.S. applications of the federal concept and describes the evolution and development of the concept of judicial review in the Canadian political arena. As well, he treats the French political heritage of Quebec with an eye to understanding how "dualism" responded to the French Canadian perception of "compact theory" and the consequent need for biculturalism: dualism was "the very basis on which French Canadians had consented to Confederation" (p. 218).

In its broader view, this book is concerned with the types of political structures appropriate for Canada today and in the future. There is much discussion of the federal model of government here and of how Canadian federalism might be modified in the future. Verney concludes that Canada's federalism today might more appropriately be described as "pseudofederalism." He argues that what is needed is "the replacement of pseudofederalism by a truly constitutional federalism with institutions appropriate for a balanced constitution" (p. 392).

This is an interesting and well-written book, which draws upon a number of different bodies of literature and should, accordingly, be of interest to several different groups of scholars. Verney's discussion of the Canadian response to "scientific empiricism" includes not only a discussion of the manner in which many U.S. intellectual predispositions differed from comparable Canadian attitudes but includes a well-written discussion of emerging Canadian political theory in its own right. His explanation of some of the implications for Canadian federalism of the British principle of parliamentary supremacy gives the reader a real understanding of both systems, as well as an appreciation for some of the more subtle problems involved in the "marriage" of different political systems. His analysis of "cultural dualism" and the French Canadian perspective on contemporary political issues does an excellent job of pulling together the many major themes of this body of literature, and clearly articulates the French Canadian position in federal-constitutional debates.

It must be noted that in one sense much of what is here is not new material. There are already substantial bodies of literature dealing with the issues of cultural dualism, biculturalism, assorted theories of Canadian federalism, the threat of U.S. cultural domination, and the like. What this book offers that is different from these other studies is the manner in which Verney synthesizes the material he presents. He brings together a number of different bodies of literature in a way that is systematic and theoretically meaningful. The material is impressively documented, and the book includes both a substantial bibliography and an index that appears to be very well done.

GREGORY S. MAHLER

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Welfare Politics in Mexico: Papering over the Cracks. By Peter Ward (London: Allen & Unwin, 1986. ix, 152p. $24.95).

In what ways and why does the Mexican state provide social welfare to low-income groups? Peter Ward argues, as the subtitle of his book suggests, that welfare allocations are designed to cover up underlying systemic failures. The state, in essence, addresses poor people's needs for its own institutional reasons. Mexican governments have no intrinsic commitment to the country's poor, even though the governing class is elected to office by a populace that, in its great majority, is impoverished.

Ward focuses empirically on the providing of land, urban services, and health care to Mexico City's poor since 1970. He bases his analysis on semistructured interviews with top officers in relevant state agencies and on a household survey of some 600 families in six low-income settlements. His methodology reflects his analytical approach. He seeks to understand the politics of social welfare by focusing not merely on the poor but also on citywide and national institutional dynamics. He rightly argues that grassroots welfare provision must be understood in the context of macro political and economic forces.

Analytically, he claims that welfare state policies are not the outcome of pluralistic interest group bargaining. Nor are they a byproduct of who runs the state apparatus, as instrumentalists argue. Rather, he supports Poulantzas's view that the state often acts against the short- and medium-term interests of dominant groups to ensure that social stability be maintained and to guarantee the reproduction of the labor force. Accordingly, social welfare allocations are costs that the state bears in order to serve the general interests of capital, and he rightly shows how politically managed such allocations are.

In conjunction with political management, Ward argues that bureaucracies can be dominated by one of two rationalities: rational-technical or partisan-political. He claims that decision making in general and social service policy making in particular have been "technocratized" since 1970. Agencies are increasingly staffed by technocrats. Emphasis is placed on planning that makes use of technical-rational considerations, and technical-rational bureaucracies are characterized by autonomy, stability, accountability, and objectivity. By implication (though it is never explicitly stated), "technocratization" has evolved because it serves the state's concern with the long-term interests of capital accumulation.

Although his political analysis of social welfare allocations at the local level is interesting, his macro argument remains unconvincing. For one, his antiinstrumentalist thesis implies that the technocratic background of bureaucrats is no evidence that state office is used for technical-rational ends. According to structuralists, bureaucrats serve state interests, irrespective of their socioeconomic background. Second, he presents extensive data to show how politics *interferes* with the effective *implementation* of technical-rational plans and how planning initiatives are quashed. Although elite and mass support for the state is waning, it actually is the *persistence* of political bureaucracy that explains why Mexico has thus far escaped the food riots, mass demonstrations, and civil strife plaguing so many of the country's southern neighbors. Third, Ward focuses on the period in Mexican history when the state's ability to address accumulation concerns have been at a record post-World War II low. Mexico, as is well known, is suffering its worst fiscal crisis since the consolidation of the revolution in the 1930s. The reduction of budgetary allocations in recent years to low-income groups and the recent allocation of welfare benefits (in particular, the legalization of squatter land claims) in a manner that increases governmental revenue through the concomitant imposition of property taxes speak to the short- and medium-, not the long-term, class interests in behalf of which the state rules. There is no evidence—given the country's enormous public debt, the contraction of the economy, and the extensive capital flight—that whatever technocratic reforms have been implemented serve the long-term interests of capital. What needs to be explained is the *failure* of the technocratic model and the state's role in promoting structures that have undermined capital's as well as poor people's interests for the foreseeable future. Ward has obviously bought the ideology and world view of the technocrats he interviewed; a "structuralist" should do better than that.

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INTERNATIONAL RELATIONS

OPEC: The Failing Giant. By Mohammed E. Ahrari (Lexington: University of Kentucky Press, 1986. 256p. $25.00).

OPEC and the Third World: The Politics of Aid. By Shireen Hunter (Bloomington: Indiana University Press, 1984. xvi, 320p. $27.50).

Mohammed Ahrari's *OPEC: The Failing Giant* is a useful and timely study of the rise and demise of the Organization of Petroleum Exporting Countries (OPEC) as the dominant actor in the international oil market. Based on the assumption that OPEC's actions can be understood largely through the analysis of underlying market conditions, Ahrari's book examines both the events that led to the 1973 oil embargo and the subsequent disarray within OPEC that produced the decline in the price of oil after 1981. Ahrari's analysis is based largely on published sources; consequently, little new material is presented in this book. The main value of the book lies in its careful, exhaustive analysis, especially of the post-1973 demise of OPEC. Unfortunately, the book was completed before the recent collapse of the oil market, rendering it somewhat out of date. A large number of statistical tables are included in the book, making it a useful reference source.

One point of criticism that can be leveled at Ahrari's study is that his insistence on the primacy of market forces rather than political preferences as determinants of OPEC's behavior prevents him from examining how economic and noneconomic factors have interacted in OPEC policy making. An exclusive focus on market forces cannot explain crucial issues such as the timing of the 1973 oil embargo, OPEC's efforts to use its oil wealth to influence the North-South debate, and the failure of OPEC members to agree on common policies regarding price and production levels. Market forces have certainly affected OPEC decision making on these issues. However, within the constraints imposed by market forces, considerable leeway has always existed for OPEC members to pursue nonmarket preferences in areas such as security and ideology. Ahrari avoids delving into the complex tangle of political factors bearing on OPEC policy making, stressing instead the role of market forces. By adopting an overly economistic approach, Ahrari fails to explain fully the complexity of OPEC policy making and misses an excellent opportunity to untangle the complicated relationship between politics and economics in the making of foreign policy.

Shireen Hunter's *OPEC and the Third World: The Politics of Aid* is a careful and exhaustive study of the various foreign assistance programs undertaken by OPEC members since the early 1970s. Because OPEC's aid giving has declined considerably since the price of oil began to fall in the early 1980s, and because she has compiled a wealth of data on the subject, Hunter is able to present a comprehensive assessment of the strengths and weaknesses of these programs. Moreover, the broad conception of foreign aid adopted by Hunter enables her to examine the general policy of the OPEC countries toward non-oil-producing Third World countries and toward the North-South debate. Hunter's book is therefore a study both of OPEC aid giving and of OPEC's attempts to influence the Third World and North-South relations.

Much of Hunter's book consists of extensive descriptions of the motivations, instruments, and specific content of the various OPEC aid programs. Since most OPEC aid giving has been bilateral rather than multilateral, much of the book therefore consists of country-specific case studies. Especially valuable are studies of foreign aid giving by Iran and Saudi Arabia, the two largest OPEC donors. Hunter's main conclusion is that OPEC aid giving has not substantially advanced Third World interests, either at the national or at the international level. Economic development in the recipient countries has not been greatly affected, and the hope that oil might prove useful in restructuring the international economic order has not materialized. Hunter argues that the ineffectiveness of OPEC aid giving has been due to many factors, including the heterogeneity and conflicts of OPEC's members, the institutional character of OPEC, the divergence of interests between OPEC and non-oil-producing Third World countries, and the international political and economic dependencies of certain OPEC countries. OPEC aid giving has therefore shown many similarities to aid giving by the developed countries. Inasmuch as OPEC's price increases have adversely affected many Third World countries, Hunter concludes that the overall effect of the OPEC price revolution has not been positive for the Third World.

The wealth of data that Hunter presents tends to overwhelm the reader, although it also makes the book a useful reference source. Hunter's data are too disaggregated in certain areas, making it difficult to relate them to the broad conclusions presented in the final chapter. Summary tables showing the distribution of OPEC aid in all its forms to individual recipient countries would have been useful in demonstrating the degree to which OPEC aid has been concentrated among a few recipients. These criticisms do not, however, detract from the overall quality of the study.

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African Crisis Areas and U.S. Foreign Policy. Edited by Gerald J. Bender, James S. Coleman, and Richard L. Sklar (Berkeley: University of California Press, 1985. xiv, 373p. $40.00, cloth; $9.95, paper).

In attempting to reconcile scholarship and policy, this volume seeks to present "the so-called Africanist case" with respect to U.S. foreign policy toward Africa (p. xiii). This "Africanist" perception emerges most clearly in a sustained criticism of the U.S. tendency to perceive African issues from the perspective of an all-encompassing East-West conflict—what is described as a "globalist" orientation. Contributors on South Africa, Angola, Ethiopia, Zaire, the Western Sahara, and Chad converge in their criticism of the U.S. tendency to view Africa through the prism of U.S.-Soviet relations. In doing so, they underline a valid point not accepted in current administration circles in the United States. Rather, these authors argue, U.S. and African interests will best be served by emphasizing the regional sources of

African problems (i.e., a "regionalist" view). Such an orientation provides us with a basis for assessing the costs of present policy directions; however, as this argument is set forth, it provides only limited insight into the process of U.S. decision making and into the fine gradations of choice open to U.S. policymakers under current circumstances.

African Crisis Areas and U.S. Foreign Policy is a useful sourcebook containing some well-researched and perceptive essays on contemporary U.S. policy toward Africa. Nonetheless it leaves a number of critical issues unresolved. For one thing, the authors do not treat the relationship between globalist and regionalist perceptions in a systematic manner, leaving unclear the circumstances under which globalism might be legitimate and/or complementary with regionalism. As Colin Legum asserts in this volume, a global approach is indispensable to the task of coping with the great economic problems besetting Africa (p. 283). If Africa requires extensive external support in terms of debt relief, appropriate economic aid and investment, and technology transfer, the problem of economic development cannot be limited to an exclusively regionalist view but requires a balanced perspective. To quote David Abernethy in an excellent concluding chapter, prudent policy entails reducing "the tension between American global and regional goals in a manner most consistent with what one defines as the interests of Africans" (p. 333). A stance reconciling constructive global concerns and regional self-determination must be found—and soon—or the interests of the United States and the African countries will be adversely affected by rising hunger, poverty, frustration and instability.

The book's focus upon African "crisis" areas opens up, as the editors tell us, a broad range of "comparative concerns" (p. 1). This raises the readers' expectations to high levels. However, two interconnected aspects soon become apparent. The crises themselves differ noticeably in terms of time horizons, the nature of the protagonists, and the intensity of conflict. And these crises are not compared in their origins, domestic and external political actors, processes of escalation, efforts at crisis management, and so forth. Perhaps the explanation for this lies with the inappropriateness of the term "crisis" itself. Are the time horizons of the sharply escalating Ethiopian-Somali encounter really comparable with the long festering struggle that besets the authoritarian and corrupt regime of Zaire? What are the thresholds between tension and crisis (p. 1), and can the term be applied to economic deterioration as well as to political/military encounters involving external as well as internal actors? (pp. 1, 323). These methodological concerns cannot be avoided if the book is to achieve its central objectives.

Other matters seem pertinent as well: the impact of conflicts within the U.S. bureaucracy upon policy formation, the significance of various U.S. interest group pressures, the contacts between U.S. nongovernmental organizations and African governments, and the role of other nonglobal actors in shaping the relations between the United States and the African countries. Certainly, this volume is a useful addition to our current materials on U.S.-African relations. I hope that it will encourage others to continue research on these subjects, in terms of generalizable and theoretical issues and in terms of more specific policy questions.

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The Reagan Defense Program: An Interim Assessment. Edited by Stephen J. Cimbala (Wilmington, DE: Scholarly Resources, 1986. xxi, 215p. $35.00, cloth; $11.95, paper).

Strategic Nuclear War: What the Superpowers Target and Why. By William C. Martel and Paul L. Savage (New York: Greenwood Press, 1986. xx, 249p. $35.00)

The Future of Nuclear Deterrence. By George H. Quester (Lexington, MA: Lexington Books, 1986. xiv, 333p. $24.95).

Quester's excellent book demonstrates the value of systematic and sustained thinking, studying, and writing about nuclear deterrence. A collection of previously published or circulated thoughts on nuclear deterrence, this volume does not avoid examining difficult questions for which there are no easy answers. Some of the specific issues Quester analyzes are the causes of war and how these are tied to defense program decisions; extended deterrence; nuclear winter; Soviet military doctrine;

substituting conventional for nuclear weapons; strengthening the defense; the future of the U.S. commitment to NATO; and trends in arms control. Analysts on the far left and the far right of the political spectrum will be unhappy with Quester's approach. He concludes that the fundamental tenets of nuclear deterrence remain unchanged, and he warns against radical new alternatives such as the Strategic Defense Initiative (SDI), deep cuts in strategic offensive forces, the adoption of a no-first-use policy, or the development of a nuclear war-fighting posture. Put succinctly, "the world must try to overcome its moral and practical problems on nuclear deterrence, for the simplest of arguments: that there is currently nothing better" (p. 119).

Attacks on the technical underpinnings of deterrence began in the early 1970s. The development and deployment of multiple independently targeted reentry vehicles (MIRVs) and the dramatic improvement in missile accuracies have eroded strategic stability because they endanger the survivability of retaliatory forces on both sides. Quester points to the dramatic improvement in missile accuracies, in particular, as leading to a renewed interest in the development of counterforce options and to a reassessment of U.S. targeting strategy, as confirmed in such documents as Presidential Directive 59. The programmatic implications of Quester's analysis are clear. Both sides should move away from MIRVs and emphasize the survivability of their missiles, rather than increased accuracy. Defense programs should concentrate on improving permissive action links (PALs), command and control of nuclear systems, reconnaissance satellites, and the security of second-strike retaliatory forces.

Martel and Savage examine the consequences when deterrence fails. In a logical and systematic fashion, they analyze the results of U.S. and Soviet counterforce attacks on their respective ICBM fields; bomber bases; submarine bases; submarines on patrol; and command, control, and communication centers. They also detail the effects of U.S. and Soviet countervalue nuclear attacks against each other's urban-industrial centers. Martel and Savage conclude that the U.S. lacks a hard-target kill capability—defined in terms of the number of available Soviet hard targets—and that the United States should reorient its planning and the configuration of its strategic nuclear forces to match Soviet counterforce capabilities.

The underlying assumption of the Martel and Savage book is that U.S. and Soviet nuclear strategies have converged around counterforce options and preemption. Like Quester, Martel and Savage attribute this convergence to technological developments that have created new targeting options. But, whereas Quester sees these trends as destabilizing, Martel and Savage argue the United States should restrict the Strategic Integrated Operational Plan (SIOP) to strategic counterforce targets and reconfigure its forces to make counterforce a realistic option. For Quester, ensuring the survivability of the retaliatory forces on both sides strengthens nuclear deterrence. For Martel and Savage, in contrast, matching Soviet counterforce capabilities achieves the same result. Thus, the programmatic implications of Martel and Savage's analysis are quite different from Quester's. They argue the United States should exchange the triad for a dyad of submarine-launched ballistic missiles (SLBMs) and bombers, concentrate its hard-target kill capability on the least vulnerable leg of the dyad—the SLBM force—and construct deep underground command posts.

Martel and Savage have done a commendable job of laying out the results of all-out strategic nuclear attacks by the Soviet Union and the United States. However, important questions about how the United States and the Soviet Union might fight a strategic nuclear war remain unanswered. For example, the authors do not address the critical issue of how a nuclear war might begin. Would a conventional war in Europe escalate to the use of tactical and theater nuclear weapons, and ultimately to a strategic nuclear exchange? Would surrogates draw the United States and the Soviet Union into a conflict in the Middle East or Persian Gulf? Would the United States be the target of a "bolt-out-of-the-blue" attack? Would the United States, under a not too incredible scenario, be the first to use nuclear weapons? How nuclear war begins is a critical question because it influences the kind of attack (counterforce or countervalue) contemplated by both sides, the scope of the attack, and the process of target selection.

Not all nuclear war scenarios are equally likely, yet Martel and Savage treat their four cases (U.S. counterforce, Soviet counterforce, U.S. countervalue, Soviet countervalue) with equal detachment. In fact, the two most likely and perhaps most interesting scenarios (a Soviet countervalue attack on the United States after a U.S. counterforce attack on the Soviet Union; a U.S. countervalue attack on the Soviet Union after a Soviet counterforce attack on the United States) are given short shrift—each is accorded only four pages in the approximately 50 pages devoted to the countervalue cases.

There are some other problems, as well, that illustrate the authors' neglect of the broader military and political dimensions of strategic targeting decisions. For instance, there is not a single mention of British, French, or Chinese nuclear forces and how these might complicate Soviet planning and targeting. The role of Pershing 2 and ground-launched cruise missiles (GLCMs) for the West is ignored; for the Soviets, on the other hand, the backfire bomber is assumed to operate only in a strategic role. Second, the authors assume that the United States and the Soviet Union would launch counterforce attacks against the same prioritization of targets, and they ignore conventional targets entirely, which could be of special importance to the U.S. Third, some of the remedies proposed (e.g., move all U.S. ICBMs and bomber bases to Maine, New Hampshire, Vermont, and New York) ignore political, economic, and social realities.

The title of Cimbala's edited collection is misleading, because only four of the eight chapters address the Reagan defense program per se. Oliver and Nathan provide a good overview of the administration's first term, and address whether the Reagan defense program represents change or continuity at a higher cost compared with the Carter program. They conclude that the Reagan program represents a change in declaratory policy but continuity in operational policy. Lawrence Korb contributes an excellent analysis of the Reagan defense manpower programs—perhaps the administration's clearest success. Ballistic missile defense and President Reagan's SDI are covered admirably by Don Snow. Cimbala's own chapter on strategic offensive modernization is the weakest of the group.

The other four chapters, two of which are excellent, review various aspects of U.S. national security policy during the Carter and Reagan administrations. Vince Davis offers a fascinating account of the defense decision-making organization under President Reagan and suggests that changes in management style influenced the substance of the administration's defense program. Alan Sabrosky considers how U.S. presidents have dealt with the war powers resolution. He concludes that Presidents Nixon, Ford, Carter, and Reagan have been quite prepared to act in their capacity as commander-in-chief and that Congress generally won't take issue with the president if the action is successful. Sam Sarkesian's chapter on special operations forces makes brief reference to the Reagan program, but focuses primarily on the general problem of unconventional war for the United States and the pre-Reagan history of special forces. The Gellner and Voas piece on arms control, in what must be a "first" in the treatment of this issue during the Reagan administration, does not mention the bureaucratic politics of the problem that have deeply colored the administration's policy.

What's missing from the Cimbala volume are important chapters on conventional force modernization (land forces, naval forces, and tactical air forces) and procurement. The goal of a 600-ship navy is mentioned tangentially in the Oliver and the Nathan and Davis chapters, but otherwise the major thrust of the administration's program to "rearm America" is ignored. In addition, the critical issues of procurement, spare parts, and the elimination of waste, fraud, and mismanagement are not mentioned. And what about the readiness and sustainability accounts, which may have been given less than their due by the administration? Finally, the editor should have included a summary chapter that ties together the issues of defense organization and leadership, program and budget, and relates these to the overall strategic philosophy of the administration.

LINDA P. BRADY

Emory University

Searching for World Security: Understanding Global Armament and Disarmament. By Curt Gasteyger (New York: St. Martin's Press, 1985. xii, 216p. $27.50).

Curt Gasteyger's new book is marked by a deep and ultimately unresolved tension. On the one hand the author makes repeated, impassioned pleas for some check on what he calls "the uninhibited militarization of our world" (p. 3). On the other, he puts forward a range of arguments for believing that no real deceleration (let alone wholesale disarmament) is even remotely likely.

In 12 central chapters, Gasteyger offers a review of recent trends globally (in terms of aggregate measures of military spending and manpower), regionally (the superpower competition, developments in Europe, Asia and the Third), and functionally (the expanding trade in arms, the prospect of further nuclear proliferation, the spread of chemical and biological weapons, the possible impact of new military technologies, and so on).

Much of what is said here seems quite sensible and it is accompanied by an array of useful tables and diagrams. Gasteyger notes, for example, that "in developing countries the use of force . . . still holds enough promise of gain and insufficient risk of punishment . . . to be considered a valid, useful, and hence legitimate political instrument" (p. 95), and he points out that "whether or not countries eventually decide to 'go nuclear' is fundamentally a political question" (p. 121).

In evaluating efforts to restrain the worldwide accumulation of armaments, the author is also, for the most part, admirably realistic. He remarks on the fading from serious discussion of calls for "general and complete disarmament" (p. 24), notes the failure of efforts at nuclear disarmament and touches briefly on the problems involved in even the narrower forms of arms control (pp. 29–30). The difficulty in each case, Gasteyger argues, is one of misplaced emphasis; "disarmament follows rather than precedes an easing of tensions . . . it is not possible in a world of tension and distrust" (p. 185).

How then are the conditions for arms limitations of any sort to be achieved? Here the author can do little more than recommend a "revolution in politics" to match the recent "revolution in technology" (p. 3). Such a transformation would depend among other things on efforts to promote "common rules of [international] conduct" (p. 195), to "improve communication, information and transparency in security-related matters" and to "reduce the role of military power as an instrument of foreign policy" (p. 198). Professor Gasteyger argues that an accumulation of such steps would amount to "'disarmament without tears,' i.e., disarmament without any sacrifice of national sovereignty" (p. 202), but it would appear instead to be a call for anarchy without anarchy, for the preservation of a system of independent nation-states somehow bleached of the most essential characteristics of all such systems.

In the end, as the author recognizes, his proposals for disarmament depend on states being convinced "that the alternative, i.e., continued armament, is likely to leave them in a much worse situation" (p. 203). But is this in fact the case? Professor Gasteyger at one point makes the familiar assertion that few countries (not including, presumably, either of the two superpowers) can feel any more secure today than they did 30 years ago (p. 20). In 1956, however, the Soviet Union was vulnerable to U.S. nuclear attack and had virtually no means at its disposal for direct retaliation. Today the Soviets have good reason to hope that their strategic forces are sufficient to deter such strikes, even if they might appear to be the only effective means of responding to a Russian invasion of Western Europe.

Some measures of the Soviet attitude on these matters is revealed in a remark attributed to former defense minister Marshal A. A. Grechko: "The more military power the Soviet Union has, the more tranquil the world will be" (p. 182). No doubt. But if the Western democracies prefer continued independence to Soviet-style tranquility they would be well advised to look to their own defenses while they pursue what meager arms limitation agreements they can and wait for Professor Gasteyger's "revolution" to arrive.

AARON L. FRIEDBERG

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Roots of Failure: United States Policy in the Third World. By Melvin Gurtov and Ray Maghroori (Westport, CT: Greenwood Press, 1984. ix, 224p. $27.95).

This is a timely book. A new interventionist spirit in the United States and the recent demise from power of two of the close allies of the United States, Ferdinand Marcos of the Philippines and Jean-Claude Duvalier of Haiti, require us to look afresh at U.S. policy in the Third World.

Roots of Failure examines the history of U.S. intervention in Iran, Nicaragua, and Vietnam to show the continuing dominance of realism and containment in U.S. foreign policy from the earliest stages of the postwar period to this day. This analysis of U.S. foreign policy covers familiar ground and draws upon the substantial political science literature on ideology, bureaucratic politics, national perceptions, and arms race theories. The three case studies of Iran, Nicaragua, and Vietnam demonstrate the U.S. proclivity from Truman to Reagan to seek military rather than political or diplomatic solutions to Third World problems and generally to back Third World right-wing authoritarian regimes in order to counter the totalitarian left.

As this book demonstrates, the postwar pattern is a familiar one. The Shah of Iran, Anastasio Somoza of Nicaragua, and various pro-U.S. regimes in South Vietnam declared an anticommunist stance in their foreign policies, requested and received vast amounts of U.S. aid, and provided the U.S. with bases, diplomatic support at international and regional forums, and investment opportunities for U.S. multinational corporations. The negative costs of these foreign policy "successes," however, were that these regimes proved to be unstable in the long term, alienated their own people and radicalized domestic opposition as a result of excessive corruption, malfeasance and human rights violations. Such regimes became liabilities rather than assets, did not serve the long-term U.S. national interests, and the United States found itself making last-ditch efforts to dissociate itself from its former friends and allies. Historically, such policies produced disastrous results as the new regimes proved to be either hostile to the United States, as in Ayatollah Khomeini's Iran, or to be Marxist-Leninist, as in the Sandinistas's Nicaragua.

The authors argue for the need to develop alternative foreign policy goals that take humanitarian needs into account. In particular, they contend that there is an urgent need for a new globalism that "would seek to get beyond national self-interest by resetting the conditions and content of international exchange, reversing and ultimately ending the arms race, and putting teeth into national and international human rights legislation—changes that could only be meaningful with the active cooperation of the United States." Drawing upon the already substantial World Order Studies literature, the authors call for substantially strengthening the authority of global institutions over global problems and for sweeping changes in the principles and practices of U.S. policy towards the Third World.

This reviewer shares the concerns of the authors of *Roots of Failure* and generally supports their recommendations. However, as the authors concede, the transition to a more humane U.S. foreign policy and to a new world order promoting human rights, basic needs, and nonintervention will not be easy. It will require radical alterations in the ideology, values, and attitudes of the foreign policy establishment. Thus, although *Roots of Failure* serves as a useful critique of U.S. foreign policy, the reader is still left wondering how to achieve a more humane world. What are needed are concrete proposals to tell us how to shift the conduct of international politics out of the hands of hard-line realists and into the hands of those who advocate the inclusion of humanitarian principles.

G. D. LOESCHER

University of Notre Dame

Economic Sanctions Reconsidered: History and Current Policy. By Gary Clyde Hufbauer and Jefferey J. Schott, assisted by Kimberly Ann Elliott (Washington, DC: Institute for International Economics, 1985. xvi, 753p. $45.00).

Academics, practitioners, and the public at large have had a long love-hate relationship with the whole issue area of economic sanctions in international relations. The love part stems from the fact that economic coercion appears to occupy that halfway house between doing nothing and using some form of military force. The hate part comes from perceptions of sanction failure, usually in the most spectacular of cases.

Interest in economic sanctions also appears to wax and wane, going up when international situations of tension arise where "something" must be done but where military or quasi-military efforts are impractical, morally unacceptable, or both. The current renaissance of general interest in economic coercion is largely the result of sanction talk (and some action) against South Africa for apartheid and against Nicaragua for a wide array of perceived sins. The past year has seen the publication of two major books on economic sanctions. The first was David Baldwin's *Economic Statecraft* (Princeton University Press). The second is the book under review here, *Economic Sanctions Reconsidered: History and Current Policy*, by Hufbauer, Schott, and Elliott. Interestingly, the two books make excellent companion pieces. The Baldwin book is largely conceptual and historical, seeking to put more rigor and less passion into the *intellectual* study of "economic statecraft." The Hufbauer, Schott, and Elliott book is much more empirical and policy-oriented. It is important for a variety of reasons, not the least of which is an attempted methodological step forward in the study of economic sanctions.

When initially hefted, the Hufbauer, Schott, and Elliott book is daunting indeed, 753 pages. Rather unconventionally, however, the "main" part of the book is only 95 pages; appendixes comprise the remainder, more than 650 pages of which are case summaries. Methodology, statistical analyses, and case compilation have been moved to the appendixes, leaving the authors stylistically and substantively free to summarize and draw conclusions in the main part of the book. Given the need for most academically oriented presses to seek wider markets, we should expect that this pattern will be increasingly common.

According to the authors, the purpose of their book is to answer three central questions (p. 1):

1. What factors in a sanctions episode—both political and economic—usually result in the achievement of foreign policy goals?
2. What are the costs of sanctions to both target and sender countries, and to what extent do they influence policy decisions?
3. What lessons can be drawn from this experience to guide the policy maker on the use of sanctions in the future?

For practitioners, the heart of the book is chapter 1 and chapter 5. Chapter 1, a brief introduction, puts the use and study of economic sanctions in historical perspective and explains where economic sanctions fit inside a foreign policy option continuum. Chapter 5, the conclusion, summarizes the results of the statistical analyses of 103 sanction episodes and offers the "surprising" conclusion that sanctions were successful in 36% of the cases. Chapter 5 also offers policy makers "nine commandments," the do's and don'ts of economic coercion: (1) keep sanction goals realistic; (2) pick on weak targets; (3) target "friends" for coercion, because allies are more vulnerable than longtime adversaries; (4) apply all available economic sanctions fully, not incrementally; (5) apply sanctions quickly; (6) beware of domestic costs of sanctions; (7) be cautious about using parallel forms of coercion—"covert action, quasimilitary measures, or regular military operations" (p. 89)—because they do not improve the chances of success; (8) do not count on multilateral sanctions, because they are difficult to organize and are probably not worth the effort; and (9) plan carefully.

For academics, the heart of the book is chapters 2 through 5, but especially the appendixes. It is occasionally on the academic side that the book is problematic, and the point on which criticism will be focused will probably be the definition of the "success" of a sanction attempt. The authors devise two simple index systems, each scaled from one to four. The first assesses "the extent to which the policy outcome sought by the sender country was in fact achieved" (p. 32), ranging from (1) *failed* through (2) *unclear but possibly positive* and (3) *somewhat successful* to (4) *successful* outcomes.

The second index reflects assessment of "the contribution made by sanctions to a positive outcome" (p. 32), ranging from (1) *zero-to-negative* through (2) *minor* and (3) *modest* to (4) *significant* contributions. The two scores on any case are then multiplied to obtain a 1–16 "success score" (p. 33). That is about all the explication offered.

Other indexes are created for the variables "international cooperation" with the sender (pp. 34–36), "economic health and political stability" of the target (pp. 36–37), and "prior relations between sender and target" (pp. 37–38). The authors also make a noteworthy attempt to assess the solely *economic* costs of a sanctions episode for both target and sender (pp. 38–39, 60–69, and appendix B). So much, including conclusions drawn from the key multiple regression analysis, depends on the "success score" that the brief discussion leaves this reviewer uncomfortable. It also brings up the issue of using case studies as the source of the data.

For the basic data, the book relies on analyses of 103 sanction cases from 1914 (U.S. versus Germany) to 1985 (U.S. and others versus Grenada). This approach, however, is a classic two-edged sword. On the positive side is the fact that the authors have satisfied a basic methodological tenet. They have compiled a respectable number of case studies, asked a consistent set of questions of each and statistically analyzed the results. On the negative side, the case evidence is highly uneven in quality, being more suspect obviously in recent cases. That is, the basic building blocks of the statistical analyses differ in size, shape, and quality of material. I foresee a significant number of dissertations focusing on interesting but inadequately documented "sanction episodes" among the 103 cases. The authors must be commended, however, for actually doing what most others simply talk about, turning case studies into data.

In addition to a too-abbreviated discussion of "success" and of methodologies in general, the book does not deal with two dimensions of economic coercion that probably represent the next generation of scholarship in this area: (1) the deterrent utility of economic sanctions and (2) the role of positive sanctions (rewards) in foreign policy success. Of course, both issues pose methodological difficulties that make those of Hufbauer, Schott, and Elliott pale by comparison. Nonetheless, conceptual and empirical analyses on these two issues are vitally necessary to round out exploration of economic sanctions in international relations.

On balance, *Economic Sanctions Reconsidered* is provocative and should be read by anyone interested in the role of economic coercion in international relations. Practitioners will find it informative. Academics will take their shots at conceptual definition and methodology, but Hufbauer, Schott, and Elliott must be recognized as having provided a courageous point of departure for rigorous empirical work on economic sanctions.

RICHARD STUART OLSON

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African Regional Organizations. Edited by Domenico Mazzeo (Cambridge: Cambridge University Press, 1984. ix, 265p. $42.50).

Famine, military coups d'état, and rhetorical posturing seem to be the staples out of which the mass media (aside from Hollywood historical dramas with colonial settings) fashion popular images of Africa. Notwithstanding the threnodies about the continual violence occurring in the black townships of South Africa, African politics can be quite placid and unglamorous, even in many government ministries and bureaus. This particular work provides a needed corrective to fuzzy and incomplete popular writing about African states and their relations with African and extra-African states. The editor and his nine colleagues, who teach in U.S., Canadian, and east and west African universities, have furnished an exceptionally well-arranged analysis of the transnational organizational politics of sub-Saharan Africa. All of the contributions are original essays, and all are admirably linked through an exemplary bibliography of books, articles, and unpublished materials in English and French dealing with African regional organizations.

Thanks to a smooth, articulate editor, the reader need not be an expert in either the African politics or international organization fields. Professor Mazzeo surveys the post-World War II decolonization era as it relates to Africa in order to demonstrate why the early federal approach (favored most visibly by Kwame Nkrumah of Ghana) was displaced by the functional school of international organization. Africa, he notes, was receptive to extracontinental ideas of cooperation given the interest of African leaders in the results of Western European economic, political, and security cooperation, if not integration. This receptivity was an indicator of the extent to which these leaders had become devotees of the creed of pan-Africanism, a creed that offered the prospect of pooled strength from individual national weakness. This collection of essays, avers the editor, will "provide the reader with a better grasp of the possible complementarities between continental and subcontinental cooperation as instruments for building and strengthening the national capacity for development and for helping restructure the international system" (p. 9).

Four authors consider organizations at the continental level (the United Nations, the Organization of African Unity (OAU), the African Development Bank, and the Economic Commission for Africa). Leon Gordenker observes how skillful African diplomats are in using the UN "as a negotiating theater" (p. 24), while he correctly emphasizes the welter of differing concerns that split the African group, especially once they have dispensed with the unifying issue of African majority rule in Southern Africa. The OAU is an even weaker and more impoverished ensemble of states. K. Mathews suggests that OAU members were captivated with "the accelerated struggle for national identity, [and] the struggle for . . . consolidation of internal political power and gaining political influence in relation to their neighbours. Consequently, there seemed to be little time and energy left for such remote concerns as the OAU" (p. 78).

Five of the authors consider the record of regional organizations at the subcontinental level. These organizations are the defunct East African Community, the Customs and Economic Union of Central Africa, the Economic Community of West African States, the francophone African and Malagasy Common Organization, and the recently inaugurated Southern African Development Coordination Conference (SADCC). It is at this level that one observes less flamboyance as well as marked foreign economic intrusion, especially in francophone and Southern Africa. Peter Meyns suggests that "the prospects for the viability of SADCC within the framework of what it can realistically hope to achieve . . . seem quite good" (p. 219). In light of the intense pressure on South Africa's trading partners to implement economic sanctions, interest in the viability of the SADCC is nothing less than prudence. This loosely structured group of none internationally recognized states is attempting to diminish dependence upon South Africa in an incremental, sectoral fashion, while withstanding South African attempts at destabilization.

RICHARD DALE

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Cyrus Vance. By David S. McClellan (Totowa, NJ: Rowman & Allanheld, 1985, ix, 194p. $22.95).

Henry Kissinger: Perceptions of International Politics. By Harvey Starr (Lexington, KY: University Press of Kentucky, 1984. xiv, 206p. $23.00).

David McClellan's *Cyrus Vance* provides us with an overview of the man, his education, early law career, and experience as a diplomatic troubleshooter. The logical choice for secretary of state, Vance shared the president's desire to promote a better world order and was perceived as a team player who would fit well into Brzezinski's notion of a balanced leadership in the area of foreign affairs.

Yet, as the details given in this work suggest, Vance was never to penetrate into the president's inner circle. Carter's moralism, as McClellan argues, left him impervious to the constraints that would undermine some of his most ambitious projects, while Vance's temperament and earlier diplomatic experience led him to face these limitations. This difference, plus Vance's dislike of abstractions, undermined his influence with the president, who liked grand plans. There were fundamental conflicts, too, with Carter's national security advisor. Zbigniew Brzezinski relished confronting the Soviets, while Vance was deeply committed to finding common ground with them so that an arms limitation agreement could be signed and the general level of hostility reduced. Thus, Vance opposed the deep cuts in nuclear war heads in the president's first arms limitation proposals to the Soviets against the advice of Brzezinski, who seemed to relish the thought that the Soviets would reject them. Later, Vance's diplomatic efforts to isolate problems due to Soviet involvement in the Horn of Africa from the strategic arms limitation talks were undercut by Brzezinski's public statement linking Salt II to the issue. Beyond this, Brzezinski saw radical nationalistic movements as working to the Soviet advantage, while Vance believed that nationalism would override ideology and that the national interest need not lead the United States into opposing radical movements throughout the world. Brzezinski was inclined, also, to opt for the use of force whenever he thought U.S. interest was endangered, while Vance was much more inclined to opt for diplomatic means.

Yet, Vance's tenacity and attention to detail were crucial components in two of Carter's great foreign policy accomplishments: the Panama Canal treaties and the Camp David negotiations on the Middle East, with the subsequent Israel-Egyptian Peace Treaty. Vance, urging a country-by-country approach, also brought to Carter's human rights policies a balanced and prudent appreciation of the limits of U.S. power and wisdom.

Overall this is a well-written, well-argued book that accomplishes what it sets out to do, namely, provide a concise overview of the contribution of one secretary of state. The sources are mainly memoir materials, published articles, and a few interviews with Washington influentials, including Cyrus Vance.

There are some matters, however, that McClellan does not deal with that this reviewer would like to have seen. Somewhat surprisingly, given the author's emphasis on Vance's character, is his nearly complete neglect of the decision-making process that led to the Iranian hostage rescue operation and Vance's extraordinary resignation from office. Indeed, one can obtain a better feel for the moral courage of the man from Hamilton Jordan's recollections of the secretary of state's opposition to that venture, *Crisis* (Putnam, 1982) than in this work. McClellan could also have better explained the factors limiting Vance's influence on Carter if he had gone beyond face evaluations of Carter's personality and his relationships to others. When one looks at some of the decisions that created political problems for Carter—the grandiose proposals to the Soviets for deep strategic arms cuts, the 180-degree turn on the neutron bomb, his initial stubborn insistence on the withdrawal of troops from South Korea—one finds not an excess of morality, but Carter's extraordinary pride—in other words, his desire to do very important things and not be shown wrong. Vance, emphasizing limits, did not fit in well with those proclivities.

Finally, a closer analysis of Carter's earlier career and rhetoric would suggest that in fact —if not in presidential rhetoric—Carter was really more predisposed to follow Brzezinski's tough line against the Soviet Union than Vance's more moderate approach. As I have shown in *Jimmy Carter: In Search of the Great White House* (Norton, 1980), Carter had been a leader in the "Anyone But McGovern" movement in 1972, and in his speech nominating Henry Jackson for president, he had called for a U.S. arms buildup. Carter had even run advertisements in the South during the 1976 campaign, attacking the Ford-Kissinger detente policies and aligning himself explicitly with Ronald Reagan. Carter's failure to back Vance against Brzezinski's public Soviet baiting, his tilt towards playing the China card, and his linking Salt II to events in the Horn of Africa—all were in tune with the deeper instincts Carter had suppressed in his efforts to win the presidency in the post-Vietnam mood of 1976.

In *Henry Kissinger*, Harvey Starr delineates the impact of Kissinger's personality and operational code on his attitudes towards the USSR and the People's Republic of China. To do this he uses a variety of techniques. In part 1, he draws from traditional biographies, psychobiographies, and Kissinger's own writings before taking office to delineate Kissinger's personality and basic world view (i.e., *operational code*). As he shows quite convincingly, the traumas of Kissinger's early life experiences —the flight of his family from Nazi Germany when he was fifteen and the need to make his own way in an unfamiliar U.S. culture with no father to pave the way for him—led him to a view of the world in which disorder is always threatening to overwhelm order and power must be obtained to prevent that from happening. An extraordinarily able young man, he found patrons to help him attain that power and his subsequent historical and political analyses were elaborations of his basic premise that the preservation of order is the highest political value.

In part 2, Starr concentrates on one aspect of Kissinger's belief system, his images of the enemy. Employing quantitative techniques (first developed by psychologist Charles Osgood) to Kissinger's public statements from 1970 through 1976, Starr shows that Kissinger had a relatively open belief system. Unlike Dulles, to whom Ole Holsti earlier applied this technique (D. J. Finlay, O. R. Holsti, and R. R. Fagan, eds., *Enemies in Politics* [Rand McNally, 1967], pp. 25-96), Kissinger did not see an insatiable and evil USSR facing the U.S. His anti-Soviet statements came after Soviet forays into the Horn of Africa and were a response to their challenge to the principle of order. As for the People's Republic of China (PRC), Kissinger's perceptions were inversely related to his perceptions of the USSR. As he came to distrust the Russians, his friendliness towards China increased, belying his earlier statements that the U.S. should never try to play off one country against another in a triangular balance.

The relevance of Kissinger's perceptions of the USSR and China to U.S. policies is analyzed by relating these perceptions to events data as measured by the Azar Sloan scale for the period from 1972 to 1974. Starr concludes that there is no clear relationship between Kissinger's negative perceptions of the USSR and U.S. policies. Perceptions have an indirect relationship to behavior, he concludes, the latter being influenced by a number of other variables. Finally, Starr shows that no important changes in Kissinger's perception of the USSR or the PRC were related to changes in his role as he moved from being the national security assistant to his position as secretary of state under Nixon, then Ford. From this finding, Starr speculates that idiosyncratic factors may be more influential than *role* for a high-level decision maker.

Through the use of quantitative techniques, Starr gains a precision in his analysis and an ability to make comparisons with greater certainty than would be possible with a more qualitative study. It is one of the best studies in this genre that has been produced in recent years. When the book is compared to McClellan's, however, it is clear that in the quantitative section of this work, Starr sacrifices, to some extent, contextual variables, facility of expression, and breadth of judgement. For the qualitatively oriented scholar, it sometimes appears that he uses extraordinarily complicated scientific techniques to arrive at conclusions that could be more simply demonstrated. Moreover, there is a problem in his research design. His conclusion that Kissinger's views of the USSR did not have much of an impact on policy may very well be due to the time frame he used (which is prior to Soviet involvement in Angola) and the short (two-month) lag between event and perception. Certainly, Kissinger's negative assessment of Soviet involvement in the Horn of Africa contributed to a political climate that by 1976 would make detente a dirty word in U.S. politics. Finally, Starr's speculative generalization about the relative unimportance of *role* is not persuasive to those who take a broader historical perspective. There were no really important differences in the powers he had in each role. A comparison between Kissinger and Vance as secretary of state would be more instructive. One man operated in a political setting that gave him primacy in the formulation of foreign policy, the other in one in which there were strong competitors, including the president.

Both of these works deal with an important subject too often neglected in political science, namely, the importance of the individual in foreign policy making, and both do well what they set out to do. Each shows the strength and weakness of its genre. The McClellan book covers more material and excels in facility of presentation. The Starr book goes further in its attempts to develop both general theory and the quantitative techniques that permit greater certitude of research results. A broader historical perspective would have increased the sophistication of each author's interpretations of one or two significant matters.

BETTY GLAD

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Verification and Arms Control. Edited by William C. Potter (Lexington, MA: Lexington Books, 1985. xiii, 266p. $29.00).

This is one of the first works to address the problems of arms control verification—a particularly pertinent issue area now, given the Reagan administration's emphasis on it. The book is the product of a conference held in 1984 and brings together chapters that fall into two general areas: the capacities and limitations of verification technologies and the political implications raised by both verification capacity and its results.

Chapters addressing verification capacity include Warren Heckrotte's discussion of the monitoring requirements for the various test-ban proposals, William Durch's consideration of Antisatellite Satellite (ASAT) control and its monitoring difficulties, Jeffrey Richelson's elaboration of U.S. verification technology, and Dean Wilkening's chapter on monitoring cruise missiles and bombers. Each of these areas demonstrates the limits of verification capacity, ranging from modest (in the test-ban treaty area) to serious (in the ASAT area, due in part to the number of potential ASAT weapons, and the bomber and cruise missile categories). These problems suggest that arms control efforts in such areas might profit from the suggestions found in James Schear's chapter on cooperative measures, even though these measures offer only limited benefit.

The political implications of verification include such things as compliance standards, superpower cooperation on verification issues and results, and the impact of domestic political change. On this last point Michael Krepon discusses changes in the verification issue in the U.S. from the looser "adequate" verification of 1963–1969 and the tougher "effective" verification standards of the Reagan administration, coupled with a more public airing of suspected Soviet violations. Dan Caldwell notes how this shift has taken place on Standing Consultative Commission issues, claiming that until recently it performed a useful role in clarifying and resolving arms control compliance issues. Mark Lowenthal and Joel Wit note, though, that some of these changes come from vague verification standards conflicting with more stringent verification requirements brought about by increasingly complex arms control agreements.

It is easy to simplify verification difficulties —some charge that the Reagan administration uses them to legitimize its disinterest in arms control, while others claim that the Soviets will cheat on arms control provisions whenever they can. But, as Allan Krass notes, there are fundamental differences in the ways the two sides view verification issues, even to the extent of how they are sequenced in arms control negotiations. Each side fears that verification permissiveness will simply enhance the other side's ability to carry out espionage. And in some cases there are practically no agreed-upon means of verifying compliance with agreements. F. R. Cleminson notes this with respect to limits on chemical and biological weapons.

None of the chapters is excessively technical —a boon for the reader who is not so inclined. Of course, this means that some of the more interesting technical details of the monitoring systems are omitted, which may leave some readers wondering just how the systems work and how reliable they really are. Security classification may be responsible here.

The editor might have provided a more comprehensive introduction, linking the political-strategic with the technical aspects of verification, thus integrating more fully the disparate chapters in the collection. But Potter can be commended for bringing together a distinguished group of specialists whose contributions collectively illustrate both the limitations and opportunities for verification as a part in the arms control process. He has chosen a fairly homogeneous group. It would be fair to characterize the contributors as arms controllers, who would probably be uncomfortable with the present administration's absolutist approach to verification questions. They collectively make a good case, though, that there is sufficient complexity in the entire issue area to prevent strict judgements as to either compliance or violation of arms control agreements.

DAVID S. SORENSON

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Camp David: Peacemaking and Politics. By William B. Quandt (Washington, DC: Brookings Institution, 1986. xvi, 339p. $32.95, cloth; $12.95, paper).

William Quandt, then a senior staff member of the National Security Council, played an important role in shaping the Carter administration's Middle East policy and was an active participant in all the events leading toward and occurring during the Camp David negotiations.

In addition to his own observations, Quandt consulted relevant, still-classified documents at the Carter library and he carried out extensive interviews. The result is a detailed and judicious assessment on the process of making Egyptian-Israeli peace although the book also provides a broader history of Middle East policy during the first half of the Carter presidency. The Camp David talks are of the greatest interest not only as an important political breakthrough in the Middle East context but also as a high point in the management of the policy process itself—with successful coordination of all parts of the executive branch—and as a case study in diplomacy equivalent to the Cuban missile crisis or the Nixon administration's opening to China. Since the book is so clearly written and each stage of the developments is so well explained, it will be of tremendous benefit for courses on international affairs and contemporary Middle East politics.

After an excellent background discussion on the pattern and timing of presidential interventions in the Middle East, Quandt sketches the views of the main U.S. policymakers. This survey demonstrates particularly the administration's eagerness for diplomatic progress in the Middle East and its willingness to devote so much of its time and resources to the issue. But such energy in and of itself did not suffice. Attempts to reconvene the Geneva conference tended to become an end in itself. An initiative designed to win PLO acceptance of UN Resolution 242 used much of the White House's political capital and (like later efforts in this direction) got nowhere in the face of the PLO's intransigence.

President Anwar al-Sadat's dramatic announcement of his willingness to visit Israel broke the logjam, showing the necessity of the local states taking the lead in making any progress on the Arab-Israeli conflict. But the administration also soon concluded that the initiative could succeed only with further U.S. involvement. "For Egypt and Israel, it is fair to say that peace was possible, but not inevitable, after the October 1973 war" (p. 4). Of course, Carter realized, in Quandt's words, "the traps and dangers of playing the role of intermediary between two highly suspicious adversaries, one of whom had a strong constituency in the United States, and the other of whom was rapidly becoming a media star of consequence" (p. 117).

While the talks themselves were conducted very well, the U.S. side did not fully understand the regional context, believing, Quandt notes, that "Egypt was the key to war and peace in the Middle East. If Egypt chose peace, other Arab states would eventually follow" (pp. 1-2).

The problem of the aftermath of Camp David is that an Egyptian-Israeli agreement could not bring about the participation of other elements, including the Palestinians, Jordan, or Saudi Arabia. The point was that peace was only possible—no matter how energetic and innovative U.S. involvement might be—when the parties involved were ready to make peace. These latter Arab forces were not then prepared to do so and they have remained unable or unwilling to take the necessary steps despite such efforts as the Reagan plan (1982-1983) and King Hussein's attempts to assemble a Jordanian-Palestinian joint delegation (1985-1986). While, as Quandt argues gently—and other critics more vociferously—Carter might have better handled his follow-up campaign to the agreements, Arab unwillingness or inability to step forward and the continuing veto exercised by the PLO and Syria against direct negotiations, have been the main barrier to progress.

There seems to be a peculiar U.S. attitude toward diplomacy that is seen with particular frequency in discussion of Middle East negotiations. Rather than perceiving long-range trends and efforts that build upon one another, observers argue that any initiative or step that does not bring an instant breakthrough is, in fact, a failure. This kind of thinking has led to some dismissal of the progress achieved at Camp David. Nonetheless, as Quandt concludes, "the two agreements reached at Camp David marked an important watershed in the peace negotiations, but a long road remained to be traveled before peace would actually be achieved. Along the way there would be pauses, detours, some backtracking, and many dead ends" (p. 259). This well-written and judicious book will probably remain the definitive work on one of the most important landmarks on that road.

BARRY RUBIN

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The Ties That Bind: Intelligence Cooperation between the UKUSA Countries. By Jeffrey T. Richelson and Desmond Ball (Boston: Allen & Unwin, 1986. xviii, 402p. $29.95).

During the investigation of the federal intelligence agencies by the Senate Select Committee on Intelligence (the Church Committee) in

1975, senators often wished they knew more about how intelligence worked in other democracies, the better to evaluate how it ought to work in our own. Yet, little information was available to answer this question. Now a solid comparative analysis has been undertaken by Richelson and Ball.

Part 1 of the book instructs us in the organization and mission of the intelligence agencies in Britain, Australia, New Zealand, Canada, and the United States—combined, one of the largest bureaucracies in the world with a budget of some $16–18 billion and authority to use severe coercive powers. With these "wiring diagrams" in place, the authors proceed to the central question of how the agencies operate both at home and within the U.K.-U.S.A. (UKUSA) relationship. Separate chapters on signals intelligence and ocean surveillance illustrate the impressive—some would say frightening—capacities of these agencies to track the movements worldwide of friend and foe.

For political scientists, the key portions of this study are those which focus on the discord and collaboration among these agencies. We have long known that the U.S. intelligence agencies often resemble embattled feifdoms, warring among themselves over budgets and missions; here we learn that—small comfort—the intelligence establishments of other democracies are as fragmented as our own. As for relations between the UKUSA nations, the authors state that, despite much sharing of valuable information, "there is also an enormous amount which is not fully shared among the five countries" (p. 168). In some cases, the need-to-know principle excludes certain countries (CIA paramilitary operations in Nicaragua are of little relevance to New Zealand). Agencies also refuse to reveal, even to friendly nations, sensitive *modus operandi* or the identity of agents. And, yes, Virginia, the allies spy on one another (though with more restraint than against enemies), and, obviously, they do not disclose these operations either.

The authors, in a word, provide a unique survey of the hidden side of five democratic governments. Sometimes the study reads too much like a survey, a description of each item in the pantry, when one might hope for more evaluation, even theorizing. We are told, for example, that the President's Foreign Intelligence Advisory Board (PFIAB) "has the authority to review the performance of all agencies . . . [and has] a full-time staff and consultants to conduct special inquiries" (p. 130). This leaves one with the impression that this panel really plays a significant role. Harry Howe Ransom's evaluation of PFIAB as more "a polite alumni visiting committee than a vigorous watchdog" (*New York Times Magazine*, 21 May 1961) remains closer to the mark. The inevitable error or two also creeps into the work. The late William C. Sullivan of the FBI is charged with writing an anonymous letter to Rev. Martin Luther King, Jr., encouraging his suicide (p. 294). This despicable event in the Bureau's history certainly happened, but whether Sullivan was the perpetrator was never established with certainty by congressional investigators. These are minor distractions, though, from a work that is commendable for its scope and thoroughness of research in a field where information is difficult to obtain.

The authors are sensitive to the danger these powerful agencies pose to liberty. They write that "many members of the UKUSA security services are unable or unwilling to distinguish between protest and dissent on the one hand and subversion and disloyalty on the other" (p. 306). To guard against this danger, the authors advocate "firm, continuous and responsible oversight and direction" (p. 307) and a citizenry "fully apprised of the nature and operations of these agencies" (p. 309). The authors have made a significant contribution toward this laudable goal of an informed citizenry.

LOCH JOHNSON

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Lawmaking and Co-operation in International Politics. By David Sanders (New York: St. Martin's Press, 1986. xi, 161p. $22.50).

It is perhaps ironic that, at a time when realism (in its classical or neorealist form) has been generally reaffirmed as the dominant paradigm in international relations, international law—a subject so closely associated with the idealist paradigm—has been enjoying a modest revival. Of course, Kaplan and Katzenbach long ago showed how international law, rather than representing a pie in the sky, Pollyannish approach to world order, was not only compatible with realism but was grounded in it. More recently, Gilpin and others who have explored the "hegemonic stability theory" of regime change have likewise shown how international politics and international law can be viewed as two sides of the same coin.

David Sanders has produced a monograph that also seeks to examine international law in a realistic fashion, accepting the realist proposition that international politics is driven primarily by realpolitik considerations but arguing that international law can also play a role in shaping interstate politics. In particular, his "reconstructed idealist" hypothesis posits that the more frequently countries engage in law making (treaty making), the more likely they are to develop a degree of mutual trust that will reduce the possibility of their resorting to armed force against each other in the future. It is the law making or treaty making *process,* more than international law itself, that is a source of peace. He submits this hypothesis to empirical testing through both an elaborate data analysis of dyadic treaty making and war making patterns in the interwar period (1920–1942) and a detailed case study of Anglo-Turkish relations during the interwar years. The findings provide partial support for Sanders' thesis, his major conclusion being that "in certain limited contexts in the interwar period . . . in situations where disputes between nation-states were less intense and less in immediate need of resolution by force than they were at the (European) core of the international system, the employment of the treaty making process did indirectly reduce nation-states' tendency to use force as a means of settling the conflicts of interest between them" (p. 67). The author found that the relationship between treaty making and war avoidance was strongest in those cases where pairs of states were distant geographically, culturally, and in terms of power. Anticipating the criticism that his reconstructed idealist hypothesis is tautological and given to spurious correlation, Sanders conducts a variety of tests to demonstrate that the proclivity of states to enter into treaties can be treated as a causal factor in their subsequent war avoidance rather than being dismissed merely as a manifestation of already trustful and peaceful relations.

Sanders is to be commended for adding to a growing literature in recent years that, departing from the heavily philosophical, legalistic, and moralistic tradition of the idealist school, has sought to subject international law to rigorous and systematic empirical analysis in order to increase understanding of the actual as opposed to the hoped-for role of international institutions. Notwithstanding the author's obviously considerable skills at quantitative and historiographical methodology, however, the reader is left unpersuaded by both the theoretical argument and the evidence. As a theory of how habits of cooperation can be learned and trust can be developed among states, leading eventually to peace between them, Sanders' thesis sounds strikingly reminiscent of notions advanced by the likes of Mitrany and Haas (functionalism), Deutsch (transactionalism), and Osgood and Rapoport (game theory), but is not nearly as well developed and sophisticated. Although the study assumes that "the most obvious and potentially fruitful 'non-fatal if unsuccessful' co-operative strategy available to nation-states in the contemporary international system is the bilateral . . . treaty-making process" (p. 2), why should one think that treaty making is a particularly key engine for building trust and peace, as distinct from the cooperative dynamics represented by international organization membership, regime compliance, expanded transaction flows, or Graduated Reduction in Tensions (GRIT)-type confidence-building diplomatic initiatives? As an attempt to provide a stronger empirical foundation for "reconstructed idealism," the study is too narrowly couched either to validate idealist thought or to qualify the validity of realist thought. In any case, one does not have to be a "fanciful optimist" or to have read this book to believe that "cooperative strategies in international politics are at least worth trying" (p. 1).

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The Vietnam War: A Study in the Making of American Policy. By Michael P. Sullivan (Lexington: University Press of Kentucky, 1985. 198p. $20.00).

Michael Sullivan is the author of a well-known book surveying the contributions of empirical political science to the study of international relations. In this book he continues his quest for a more scientific, theoretical approach to international relations by examining the Vietnam War. Although he does have policy goals and interests as well—to help focus attention on what he sees as the neglected lessons of the war for U.S. foreign policy—his primary goal is to use the war as a case study to test several theories about U.S. foreign policy. Still more broadly, by applying the social scientist's method of systematic empirical analysis to the Vietnam War, he wishes "to demonstrate that the study of American foreign policy need not be limited to the traditional diplomatic historian's analysis" (p. 6). Finally, the study is intended to bridge the gap between what the author considers to be two styles of analysis: historical examinations of single cases with no theory or quantitative analysis on one hand and highly theoretical and empirical analysis of many cases on the other.

These are worthy and ambitious goals, but the results are disappointing. For example, one chapter focuses on the well-known dispute over whether the Vietnam War is best explained in terms of the "incrementalist" or the "rational" model of decision making: did policy makers inadvertently stumble into a quagmire or did they knowingly choose what plausibly were the best means to achieve worthy goals (however badly it all turned out)? Surprisingly, the author brings scarcely any new empirical evidence to bear in his effort to shed new light on this puzzle. Sullivan's argument is obscure and lame. While leaning toward the incrementalist model, he seems mainly to be concerned that policy makers not be blamed for their human frailties and that social scientists maintain "as broad a perspective . . . as possible" and avoid "imposing one rigid model of decision making" on the war (p. 86).

The central empirical focus of the book is devoted to testing the applicability of the famous Klingberg thesis to the Vietnam War and its aftermath: does U.S. foreign policy follow alternating 20-25-year cycles of isolationism and expansion, or "introversion" and "extroversion"? Making use of public opinion data, Sullivan concludes that the Klingberg thesis has been confirmed, as the post-World War 2 mood of extroversion gave way in the late 1960s to a broad introversionist consensus that persists today. However, this analysis is questionable on at least two grounds. First, despite the Reagan administration's difficulties in getting public support for military intervention in Lebanon and Central America, it is not clear that there is really a consensus on introversion or isolationism comparable in breadth to that prevailing between the first and second world wars. Secondly, to the extent that the overall national mood of the country did shift in the 1960s, it would seem more plausible to explain this as a direct result of the unpopular Vietnam War and its disastrous conclusion rather than as a generalized, predictable long-cycle phenomenon presumably unconnected with any specific event.

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POLITICAL ECONOMY

The Political Economy of U.S. Import Policy. By Robert E. Baldwin (Cambridge: MIT Press, 1985. xi, 238p. $22.50).

Decisions taken by the U.S. government on whether or not to try to impede the flow of foreign-made goods in the world's largest single national market are significant determinants of resource allocations in the world economy and of the state of political relations between the United States and its trading partners. Import policy involves the pursuit of perceived economic rationality as well as the exercise of political power to determine who produces what; as such, it is of importance both to political scientists and economists, neither of whose disciplines by itself can fully explain the phenomenon of foreign trade policy making.

Both disciplines will be enriched by the relatively brief study of Professor Baldwin, an economist with experience in Washington's foreign trade apparatus. Nevertheless, academic reactions to this book are likely to fall into a broad spectrum between criticism and praise in accordance with readers' attitudes towards the modeling of bureaucratic behavior.

On the one hand, the author has successfully achieved his stated objective of contributing "to a better understanding of the manner in which U.S. import policies are formed and implemented" (p. 175). On the other hand, he has failed to do the seemingly impossible: to divine either a single theoretical construct of the import decision-making process or a finite set of criteria to predict flawlessly which of several models will determine behavior in a given policy-making exercise. Such a spectacular achievement may be demanded by some as the determinant of a successful new study of this subject.

Such an accomplishment is a long, long way off, and Baldwin has resisted the tack of latching onto a master theory and then selectively choosing from hundreds of case studies to allegedly verify its utility in explaining past actions. Instead, he has reached a very realistic conclusion that will disappoint those who want to bottle certain slices of human behavior into a neat quantitative formula. Baldwin calls for an "eclectic approach" to understand import policy behavior, at least until the various models are "differentiated more sharply analytically and better empirical measures for distinguishing them are obtained" (p. 180).

The book's methodology begins with a brief recitation of five models developed within the framework of public choice theory, a body of thought useful to economists trying to discern why political leaders stray from economic theory when making decisions. These models are repeatedly related to decisions examined in the next three chapters, the main body of the study. Baldwin separately examines actions in the three main institutions of import policy decision making: Congress, the International Trade Commission (ITC), and the senior levels of the executive branch. Efforts at linkage are supported by original and useful quantitative efforts to determine important variables in decision making. The author's repeated admissions that his statistical efforts have failed to produce unambiguous results will please the realists and frustrate zealous behavioralists.

Economists will not like the absence of macroeconomic considerations (e.g., large federal budget deficits and a very overvalued dollar exchange rate) in explaining recent U.S. import policies. And stylists will take issue with Baldwin's propensity for repetition in this brief book (especially the themes of his five models) and his excessive fascination with the detailed workings of the ITC.

But criticisms should not prevent acknowledgement of this very useful contribution to understanding the complex, dynamic import policy making equation that consists of different institutional dynamics, economic considerations, political pressures, career ambitions, personal ideology, and ethics. By this original linkage of a synthesis of public choice theory with an empirical review of import policy decisions, Baldwin has helped both economists and political scientists to better understand why U.S. foreign economic policy is as it is.

STEPHEN D. COHEN

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Introduction to the Theory of Social Choice. By John Bonner (Baltimore: Johns Hopkins University Press, 1986. 205p. $25.00, cloth; $8.95, paper).

Rational Politics: Decisions, Games, and Strategy. By Steven J. Brams (Washington, DC: Congressional Quarterly Press, 1985. 233p. $16.95, paper).

These two books provide disparate introductions to the microfoundations of social (political) choice. Despite a common goal, these books address fundamentally different audiences. John Bonner, an economist, successfully guides the reader through the basic tenets and problems of social choice in a noble effort to provide a comprehensive but introductory overview of a difficult subject. Steven Brams's *Rational Politics* is cast in game theoretic terms and is more appropriately read as a supplemental text by those seeking to make the leap beyond the axioms of social choice to applied examples within the political arena.

The unabashedly economic orientation of Bonner's book should be of no concern to a political science audience. The seminal works of Kenneth Arrow and Amartya Sen, familiar to economists and political scientists alike, underpin most of the theory to which the reader is introduced. The generic nature of the problem of social choice is underscored by the organization of Bonner's book. In consecutive chapters the author considers political and economic solutions to the inherent paradox confronting the designers of social choice mechanisms.

Because a true paradox has no solution per se, discussions of social choice mechanisms often lead to normative statements about the relative merits of competing alternatives. To his credit, Bonner maintains a steadfastly impartial stance throughout his presentation. While this adherence to neutrality is an admirable quality for an introductory text, it is also partially responsible for one shortcoming of the book. In his systematic presentation of the attributes and drawbacks of various social choice mechanisms Bonner fails to discuss why, in equilibrium, we observe the coexistence of mechanisms with distinctly different normative implications. Such a discussion would serve as a useful concluding or summary chapter, a feature notably absent from the book.

The relatively abrupt end of the book is characteristic of most of the individual chapters which otherwise follow in a logical, well-conceived progression. The first three chapters carefully establish the microeconomic foundations of the subsequent social choice analysis. These early chapters cover a deceptively large amount of material and should be read with care. Bonner provides short, annotated bibliographies at the end of each chapter. The inclusion of these references mutes the inevitable criticism of omission easily directed at introductory works.

The transition from Bonner's *Introduction to the Theory of Social Choice,* to Brams's *Rational Politics* requires something akin to a leap of faith. Nominally, both have an interest in explicating the process of political or, more generally, social choice. Practically, the books differ to the point of being noncomparable. Where Bonner's book easily stands alone as an introductory text, Brams should ideally be read by someone with prior exposure to social choice and game theory.

Rational Politics holds, as its basic premise, that much observed political behavior can and should be explained as the result of a perfectly rational calculus on the part of politicians or other relevant actors. To illustrate this point, Brams uses elementary game theory to expose the rationale for the outcomes of prominent political confrontations (e.g., the Cuban missile crisis). As the book progresses the reader is presented with a succession of topics, including social choice theory, that relate to rational choice models of political behavior.

My concerns with Brams's book relate to elements of both the organization and substance of the material he presents. While making a strong argument for the use of a hypothetico-deductive approach to political analysis in his opening chapter, Brams proceeds to ignore his own advice in the organization of the remaining chapters. Deductive reasoning is fundamentally axiomatic and yet it is not until the fourth chapter that any effort is given to a rigorous analysis of individual preferences, an essential prerequisite for rational choice models. The absence of such analysis prior to the presentation of spatial games in the third chapter is disconcerting.

A related concern is with Brams's development of game theory. Again we note the absence of an initial overview of game theory principles. Instead, such principles are presented over the course of the book. As a consequence, the reader must wait, for example, until chapter 6 before being introduced to basic terminology such as *normal* and *extensive form* games. The inclusion of such relatively formal terminology suggests incorrectly that *Rational Politics* is meant to serve as an introduction to game theory in addition to its role as a general introduction to rational choice political models.

My most serious concern rests with the substance of the game theory that Brams presents. As a single precautionary example, consider Brams's characterization of single-period Nash games. To claim, as Brams does, that such games are inherently myopic testifies to a profound misunderstanding of their specification.

Rational Politics works well as a testimony to the broad applicability of rational choice political models. The reader is cautioned, however, before using the book as an introduction to relevant methodology.

BRIAN E. ROBERTS

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Liberty, Market, and State: Political Economy in the 1980s. By James M. Buchanan (New York: New York University Press, 1986. ix, 278p. $45.00).

James Buchanan is a brilliant, imaginative, and prolific scholar. He is especially adept at defining terms and constructing models that reach the conclusions prefigured by his conservative ideological position, a talent amply evident in this collection of papers about the 1980s. His work is motivated and channeled by his intense dislike of the twentieth-century liberal, welfare, interest group state and by his desire to restrict both the economic role of government and its intellectual rationale, particularly in what he perceives to be redistribution. His self-expressed desire is to create an intellectual fortress supportive of what he calls the minimalist, protective, or night watchman state. For Buchanan, traditional economics is too readily interventionist. He prefers a political economy led by his "public choice" theory in providing (in the phrase of his mentor, Frank Knight) a propaganda for economic freedom.

Buchanan would prefer a world in which (1) collective political action is constitutionally limited; (2) there is minimal legal change and that, especially involving redistribution, subject to a unanimity rule (even if only implicit or conceptual); (3) there is exercised a necessarily fundamental (however limited) economic role of government, which he recognizes will channel economic outcomes; (4) the minimalist state is given constitutional protection; and (5) the constitution is necessarily subject to reform (notably, in the present world, a balanced-budget amendment). He prefers, in short, a world in which ostensibly spontaneous—rather than political—order is achieved, largely through markets, albeit within an appropriate but stable legal and institutional framework which is itself not the continuing object of change by political entrepreneurs. These are the main themes that reverberate throughout these papers.

The book is divided into five parts, covering 24 papers that are diverse and wide-ranging and not readily summarized in a short review. The papers in part 1 treat alternative perspectives on social order. Here he surveys the development of the public choice perspective and its juxtaposition, as political economy, to mainstream economics. The most important papers, first, argue that insofar as politics is perceived as constituting a quest for truth there will be a potential for the tyranny of coercion and political absolutism; and, second, make the case against various opposition arguments to constitutional reform. A unifying theme is that in both markets and politics there is no independently determined or testable efficient or correct result independent of the determining process itself. Part 2 continues the theme of "order defined in the process of its emergence" (the title of chapter 7) and explores the processes of institutional reform, especially through politics; the irrelevance (contrary to Ronald Coase) of transactions cost; and the distinction, for Buchanan, between moral community, order, and anarchy. Part 3 presents the case for a contractarian minimization of the welfare state. Part 4 presents his case against government deficits and debt and for the balanced budget amendment. He laments the putative destruction of capital values and constraint of future choices. The emphasis is on deficit financing per se rather than the size of government. Part 5 presents informal models of individual and collective choice and of democracy, all in support of the minimization of the economic role of government and legal change. Among other things, he opposes social engineering and politics conducted as the search for truth and participated in by scientists (utilizing arbitrary social welfare functions or benefit-cost analysis) in the belief that they are politically relevant truth specialists. He affirms arrangements in which voluntary exchange can predominate.

Buchanan is to be applauded for certain points that other economists, as well as political scientists and others, would do well fully to appreciate and adopt: (1) that economic performance is inexorably governed by legal and other institutions, with performance specific thereto, and that legal rights of economic significance are logically prior to the market; (2) that there is no independent basis for determining maximization: that order, utility functions, and economic performance are worked out through the processes of their determination and are not ontologically determined truths and that politics, like markets (themselves political institutions), is a valuation process, not the search for an independent truth; (3) that equal access to influence, open franchise, regular rotation of political agents, and avoidance of gross bundling of separate collective choices are useful for ex ante political equality; and (4) that much work by social scientists is a combination of preaching and social engineering, utilizing the mantle and forms of science for purposes of persuasion.

Buchanan's conservative position aside, there are numerous tensions within and problems with his arguments. While he opposes science as social control, his approach, he recognizes, also constitutes social control (p. 38). Why are his political entrepreneurship, his rational constructivism and his social engineering preferable to others? His answer would be that he wants to create institutions in which voluntary agreement predominates. But his is only a different agenda for government, and economic performance (including distribution) would be channeled by it. Individuals can always exercise voluntary (actually volitional) choice within the extant legal framework. He would argue that his is the minimalist state, but this is illusory: his state, for example, in its structuring of private power, is no less fundamental. He would minimize legal change by largely subjecting it to a unanimity rule. But it is illusory too to think that government is normatively important only in matters of legal change of legal rights. His minimal government prescription is itself but a strategem in the system of power play governing the use of government. Far from eliminating predatory politics, it would be its tool. Buchanan himself may be a romantic constructivist, perhaps even a moral anarchist (p. 119), with all the dangers of such positions.

Apropos of the view that status quo institutions and outcomes are efficient. Buchanan attempts to rebut the criticism thereof by emphasizing the possibility of institutional change. But using a greater-than-majority rule grants decisional power to a small majority and fortuitously operates in favor of the status quo (but not fortuitously for those to whom his ideas are gospel and useful). He wants to limit politics to mutual gains and does not want to interpret politics exclusively as the clash of conflicting interests. But, as laudable as that may be, social and economic conflicts exist and to ignore them is to reinforce their established resolution. For all its unattractive features, politics *is* a conflict resolution process. That it deals with choices that involve mutually exclusive (rather than Pareto-optimal) results is due to the existence of conflict. Politics is due to conflict, not vice versa, although the form that the conflict takes is influenced by politics.

Buchanan in these matters may be normatively attractive but he is so often substantively narrow or wrong that one wonders about the degree to which both wishful thinking and his conservative ideology limit and channel his analysis. His analysis is both brilliant and (in the best high-priestly tradition) casuistic—casuistic, for example, in its rejection of preferences that do not accord with his. To give him credit, their holders could in principle veto constitutional change, but he does not recognize that constitution making is itself a vehicle of power play and that unanimity rules do not in practice apply to it. Indeed, he both denies and affirms the ideological stance of public choice theory, a motivation that is a strength and weakness both of Buchanan and of the theory itself. One weakness is his own brand of absolutism; one strength is the positive hypotheses to which his ideas have given rise. The great normative strength is that he is basically right regarding the freedom-enhancing nature of competitive markets (even though here, too, he equivocates, first rejecting the structural definition of competition and later invoking it). Moreover, the book is so heavily laden with wishful thinking in a corporate system, that the tendency is for his analysis to mask and obfuscate the reality of private power concentrations and their implications for "voluntary" (actually only volitional) agreement.

All in all, this volume is conservative polemics packaged in philosophy and social science, but with much good sense and suggestive analysis for positive work. The polemics are those of Ronald Reagan; for example, the idea of the budget balance constitutional amendment. Indeed, if a constitutional convention were called on behalf of this amendment, Buchanan may well be its intellectual or spiritual patron. If such a convention attempted major overhauling of the document of 1789, it would likely give constitutional effect to his emphasis on markets and to other items on the conservative agenda, probably without the restraint required by his affirmation that politics (even constitutions, which should deal with rules and not outcomes) is in search not of truth but of conflict resolution. Indeed, pursuit of the balanced budget amendment may be the trojan horse with which to gain the opportunity for larger revision.

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Rivalry and Central Planning: The Socialist Calculation Debate Reconsidered. By Don Lavoie (New York: Cambridge University Press, 1985. 192p. $34.50).

For neoclassical economists, the market socialist controversy is an important *shared example*, to use the terminology of Thomas Kuhn. When, in 1920, Ludwig von Mises argued that rational economic calculation was impossible under socialism he precipitated a 20-year exchange that now constitutes one of the few episodes in the history of economics discussed in standard textbooks. Reading that episode as customarily presented, the student learns that a socialist economy must solve the same basic problem as under capitalism. That problem is one of efficiently allocating resources to appropriate uses, given tastes and technology known to the pertinent agents. Therefore, to prove Mises wrong, all one need do is to imagine socialist managers imitating capitalist entrepreneurs and to envisage a central planning board mimicking the free market. This shared example teaches the budding economist to disregard the constraints imposed on economic life by institutions and to assume away all difficulties connected with the diffusion of information in society.

Reading Don Lavoie's *Rivalry and Central Planning*, one can clearly see why the market socialist debate is such an instructive example for the teaching of neoclassical economics. Lavoie shows how the arguments of Mises, Hayek, and their followers in the Austrian tradition were reinterpreted in a way that would make them easily answered within the neoclassical framework. In this tradition, according to Hayek's famous 1945 *American Economic Review* article, "The peculiar character of the problem of a rational economic order is determined precisely by the fact that the knowledge of the circumstances of which we must make use never exists in concentrated or integrated form, but solely as the dispersed bits of incomplete and frequently contradictory knowledge which the separate individuals possess." The Austrian world is one of constant change, where new conditions arise every day and where the efficiency of the economic process is determined by the ability of the economic system to use the new information generated by the changing conditions. In contrast, in the economic models that are central to the neoclassical tradition, information is constant, already possessed by the relevant actors, and efficiency is defined relative to a given stock of information. Neoclassical economics simply could not be brought to bear on Mises' arguments concerning the relative efficiency of capitalism and socialism. The shared example not only instructs the student how to proceed but also gives guidance on which questions it is permissible to ignore.

Lavoie's book is the most complete record written to date of this debate. Although Lavoie writes from an avowedly Austrian perspective, his book is, with one important exception to be discussed later, a fair history of the debate. In addition to the historical record, the author provides a valuable discussion of the differences between the neoclassical and Austrian paradigms. The book is well written and the presentation is always clear. It can be highly recommended for all historians of economics, those interested in the theory of socialism, and those wishing to learn about Austrian economics.

Although recommending the book, I do believe that it has two important flaws, which some readers might find critical. First, Lavoie accepts too uncritically the Austrian view of capitalism—that unbridled entrepreneurs and unfettered markets will naturally produce the most efficient outcome. Work conducted within the Austrian paradigm may contain penetrating insights into the limitations of neoclassical theory, but that work does not constitute a rigorous alternative body of theory. Lavoie could certainly have subjected the arguments of Mises and Hayek to the same type of penetrating criticism that he reserved for Lange and the neoclassicals.

Secondly, Lavoie's book is primarily a recapitulation and reinterpretation of the historical record, not an explanation of why the debate took the course that it did. The debate is presented purely as a clash of paradigms. There is no attempt to explain why the Austrian challenge was rejected by the overwhelming majority of economists. Underneath the surface of market socialist debate, there lies the fascinating question as to why the Austrian challenge was unsuccessful even though, as the Keynesian revolution attests, the profession was open to the consideration of new alternatives. Lavoie provides the raw material on which to build an answer to this question but not the answer itself.

Despite the reservations, the book is highly recommended. But I fear there is little chance that it will be read by economic theorists. As Whitehead once remarked, "A science that hesitates to forget its founders is lost." Economics knows its way and history does not lie on the route. This is unfortunate because Lavoie's book provides an instructive lesson for the present. In the early 1970s, there began to be much debate on the difficulties that informational issues presented for neoclassical economics. But at present these issues are being sidestepped. The advent of the rational expectations revolution once more means that economists are happy to assume that all agents are adequately informed. Any study of the process of information acquisition is eschewed. If economists were to read Lavoie's cautionary tale, perhaps they would be more prepared to ask themselves whether economics is still evading the informational issues that Mises raised over 60 years ago.

PETER MURRELL

University of Maryland

The Rhetoric of Economics. By Donald N. McCloskey (Madison: University of Wisconsin Press, 1985. xx, 209p. $21.50).

Sure to be important in economics, this book should be read by political scientists interested in economics, in methodology, and in the philosophy of the social sciences. Yet the book's message and the uncommon grace, wit, and learning of its argument are such that the readers who relish it most may be those who are most skeptical about economics, methodology, and scientism.

McCloskey dubs his bête noire "modernism," the belief that the only real knowledge is that tested by "scientific," rigorous skepticism. Modernism prizes only the observable, the quantifiable, the reproducible. It aims for prediction and control and would strictly separate scientific knowledge from oughts. McCloskey draws on Kuhn and others to show that modernism is not practiced in the natural sciences. He then examines the work of influential economists and finds that they persuade not through "official" modernist means but through an "unofficial" rhetoric that uses analogy, metaphor, appeals to authority, and other methods that Cartesian doctrine should not find persuasive at all. He finds that important techniques, like simulation, are about affirming, not about falsifying. He is especially telling in his criticism, "Unexamined Rhetoric of Economic Quantification" (pp. 138–173), arguing that statistical significance should be a minor tool in the economist's box. He also offers sensible suggestions about how to arrive at usable standards of economic significance.

McCloskey attacks not just modernist methodology but all rule-bound methodology. His quarrel is not with modest, small *m methodology*—statistical theory and practice, accounting conventions, and the like. And he joins Habermas in praise of the conversational norms of civilization: "don't lie," "pay attention," "be open-minded," "explain yourself when asked" (p. 24). The problem is with capital *M Methodology* that pretends to know the route to future progress: "the modern methodologist is a Red Queen (Normative argument: off with his head), and the Gods are snickering behind their hands" (p. 20).

For McCloskey, Habermas's conversational civility aside, "there are no rules and regulations for being reasonable" (p. 52). We should be more respectful of probable argument and plausible conclusions. The standards for good reasons, warrantable belief, and plausible conclusions come "from the conversations of practitioners themselves" (p. 29). And those conversations should be open to evidence from questionnaires, introspection, the humanities, and other sources scorned by the modernist in economics. "You are more strongly persuaded that it is wrong to murder than that inflation is always and everywhere a monetary phenomenon" (p. 45). The ethical arguments for such propositions are sometimes "more persuasive, better, more probable" (p. 46) than for scientific propositions.

To those who say the abandonment of modernism opens the door to irrationalism and the mob, McCloskey says to the contrary. The "lamentable split of fact and values" (p. 124) leaves the modernist believing that reason and science do not permit him to make arguments that speak to the mob's concerns. And in the world of science the "lamentable split" makes the scientist feel that it is not quite right to try to persuade. Thus "the Announcement, the more bald, unargued and authoritarian the better, is the favored form of scholarly communication" (p. 124). Since the results are supposedly objective and statistically significant, those not persuaded must be dishonest or ideological or stupid. Modernism does not combat dogmatism and venom; it nourishes it.

Though McCloskey is usually persuasive, his desire to tweak the modernist nose sometimes leads him to overemphasize the least scientific parts of rhetoric. When, for example, he suggests that economists should defend the metaphor of the selfishly economic man as calculating machine "because of its prominence in earlier economic poetry or because of its greater congruence with introspection than alternative metaphors" (p. 82), he must realize that such arguments will *not* persuade noneconomists, nor should they. McCloskey's book focuses on economic rhetoric on the frontiers more than on why economists agree on much of importance. If, for example, he had set out to explain why a recent poll showed that only 2% of economists disagreed with the assertion that "a ceiling on rents reduces the quantity and quality of housing available" (J. R. Kearl et al., "A Confusion of Economists?" *American Economic Review* 69[2]:28–37 [1979]), I think the importance for economics of theory, prediction and evidence would have been given their due along with the neglected forms of argument McCloskey rightly emphasizes.

McCloskey thinks economics a "successful" (p. 56) science, one whose "brilliance" deserves more influence in "civilization at large" (p. 180). But his criticism of his colleagues is merciless. The "typical" economist is "harshly if unreflectively dogmatic" (p. 184). "Many are bored by history, disdainful of other social scientists, ignorant of their civilization, thoughtless in ethics and unreflective in method" (p. 7). The journals are filled with "pseudoscientific ceremony" (p. 159) and "exercises in deriving the obvious" (p. 179). Even one of the book's heroes, Robert Fogel, is praised for creating a rhetoric that brought into being a new type of reader, one "less attractive than the more common one in successful academic prose" (p. 135). And McCloskey says Fogel's rhetoric is similar to his own (p. 117)! A puzzle. One senses that McCloskey has the Habermas virtues, that he will be pleased by such questions and is intelligent enough to have some answers. We need more scholars like him and more books like this one.

STEVEN E. RHOADS

University of Virginia

The Economist's View of the World: Governments, Markets, and Public Policy. By Steven E. Rhoads (Cambridge: Cambridge University Press, 1985. xiv, 331p. $39.50, cloth; $12.95, paper).

The influence of economics in policy making inspires various combinations of wonder, envy, and disgust among some noneconomists. In this book, Professor Steven Rhoads assesses the strengths and weaknesses of microeconomic policy analysis, that is, the application of the economic theory of individual behavior and welfare to the problem of evaluating economic institutions and the policies that affect them. Along the way, he provides an excellent, detailed nontechnical explanation, with powerful examples, of some key ideas in modern economics, such as opportunity cost, externalities, the marginalist perspective, and the importance of the incentives that are created by different types of markets, organizations, and policy instruments. The book is rich in illustrations drawn from several areas of public policy, and especially good sections deal with the economics of environmental policy and income distribution policy.

The central mission of the book is to identify the weaknesses and limits of the use of economics in policy making. It considers many of the criticisms of microeconomic analysis that have been voiced by economists and noneconomists alike and in the end adopts a view that is more sanguine than that of most of the critics, as, for example, exemplified by Steven Kelman. Nevertheless, Rhoads strongly attacks several aspects of economics: the doctrine of consumer sovereignty (the presumption that individual preferences are normatively compelling), the self-interest axiom (individual behavior is motivated largely by selfish objectives), and the reigning methodology (heavy reliance on sophisticated mathematical and statistical tools to the exclusion of moral philosophy, history, and hands-on, real-world experience, as through field work).

In my view the clearest weaknesses of the book stem from its central conception, which is to deal with economists as a group of people holding shared and distinctive values, rather than with the discipline of economics as a universally applicable theory of human behavior. This approach produces several highly dubious inferences and conclusions. First, it leads to regarding anything written by any economist that is not a critique of the discipline as an expression of shared beliefs, no matter how ridiculous and ethically impoverished its content. Thus, because *one* economist once argued the point, all economists are held to believe that dueling ought to be legalized (because people duel voluntarily and there are no externalities to duels), that the direct net benefits of stealing money are zero (because the gains to the thief exactly equal the loss of the victim), and that the willingness to pay for addictions has the same normative content as the willingness to pay for prosaic goods like food and shelter.

Once one adopts this point of view, the foolishness of economics requires a "theory of economists" to explain how they could be so myopic. Rhoads offers the view that economics attracts Philistines and materialists because its content focuses on markets and financial matters. He does not tell us why this is more plausible than the view that at least a few people enter the profession because they want to use the powerful force of economic incentives, as vividly described in the book, to make meaningful progress in solving environmental problems or in improving the lot of the materially deprived. In fact, until about 1970, economists were more likely to be criticized for being iconoclastic liberals than for exhibiting conservative materialism, and one wag advised that the route to success in economics was to travel to Cambridge and turn left.

Political scientists are likely to find especially interesting the final section of the book that deals with the application of microeconomics to the study of political processes. Here the reader should be warned that Rhoads's characterization of the economics literature is idiosyncratic, for his coverage is limited essentially to the public choice school of James Buchanan and Gordon Tullock. While major figures in the economics literature try to develop a positive theory of government, I find it quite misleading to place such heavy stress on their work while all but ignoring, say, Kenneth Arrow, who not only approaches the subject in a far different way, but who also won a Nobel prize for this work. Even more misleading is to weave together the positive economic theory of Buchanan and Tullock, and, to a lesser extent, George Stigler, with their political philosophy. The first error is that their philosophy is more conservative than most of the writings on the topic that are uncited, but the bigger error is the implication that conservative political philosophy is inextricably linked to an economic theory of politics. The book elsewhere contains some of the evidence on these points in its summary of the economic literature on environmental policy and income redistribution, much of which can hardly be classified as politically conservative.

In discussing the positive economic theory of political behavior, Rhoads takes issue with the core assumptions that politicians attempt to maximize the probability that they will hold office, that political participation is motivated by self-interest, and that one should be highly skeptical of assigning benefits to actions that attempt to impose one person's preferences on another. All these issues need to be confronted, of course, but the argument in the book is incomplete in some respects. Although Rhoads is correct in identifying consumer sovereignty as the soft underbelly of benefit-cost analyses that are based on the willingness-to-pay of citizens, the book does not explore whether this has any implications at all for political decision processes. The core issues are whether consumer preferences are manipulable and whether a society that bases public decisions on selfish preferences thereby must encourage materialism among its citizens. The problems seem obvious. Political preferences, too, may be manipulable. And political actors, not economists, decided to institutionalize economics into government decision processes, which some might see as an *effect* of the materialistic nature of a society, rather than a *cause.* In any case, the extensive analysis of how government officials ought to make decisions serves to confuse the positive and normative aspects of economics. Benefit-cost analysis is normative, in that it leads to the recommendation of one course of action over another. The economic theory of political behavior is positive, in that it yields testable conclusions about the effects on public policy of changes in the economic environment and the political institutions that make policy. It is, of course, quite incorrect to say that the perspective *recommends* that politicians maximize reelection prospects or that voters evaluate candidates on the basis of the personal benefits the candidate will provide them.

While most any economist will certainly find these and other faults in Rhoads's work, the book nevertheless is a valiant attempt to attack a difficult issue. It is tough enough to come to a deep understanding of another discipline, but to have something sensible to say about its strengths and weaknesses is a herculean challenge. And many of the ideas and observations in the book are quite penetrating. It is definitely a book that a person who tries seriously to undertake multidisciplinary work between economics and political science ought to confront, for it usefully indicates the many ways one can display narrowness or overlook important causal forces in such endeavors.

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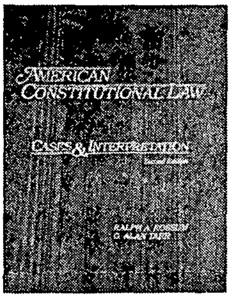
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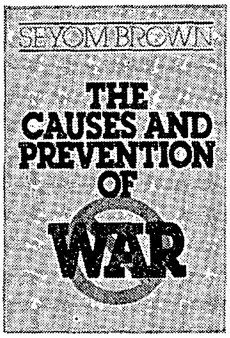
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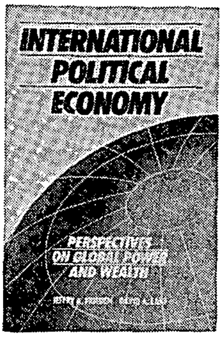
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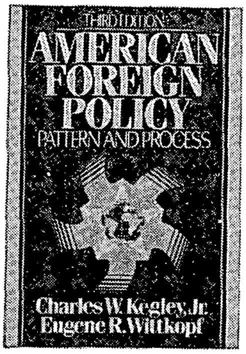
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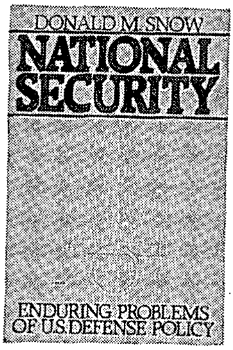
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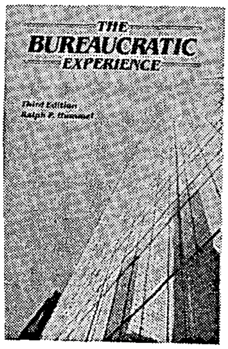
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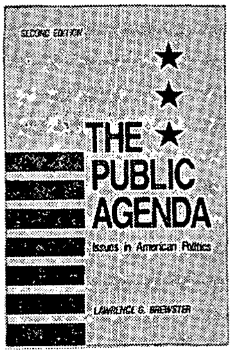
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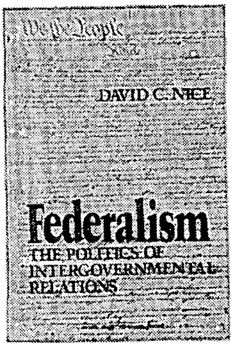
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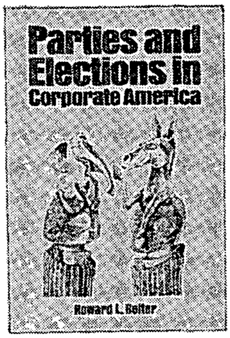
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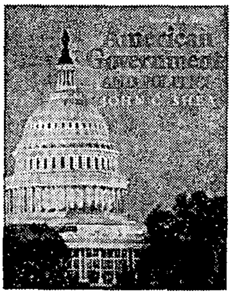

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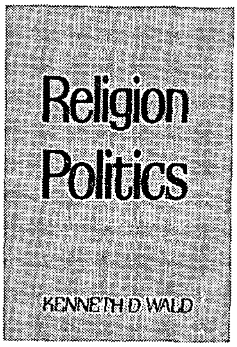

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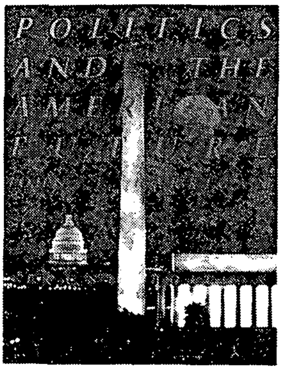
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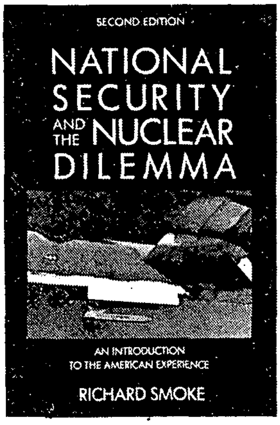
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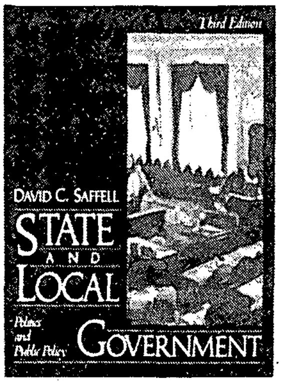
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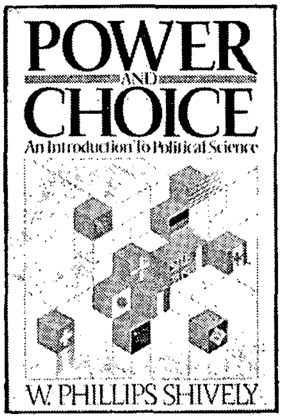
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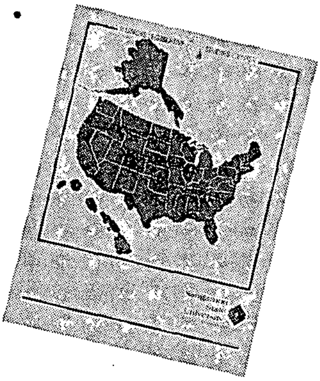

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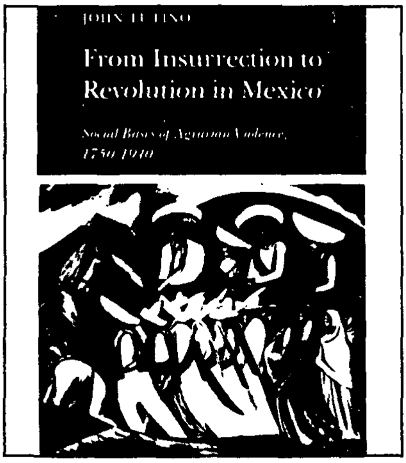
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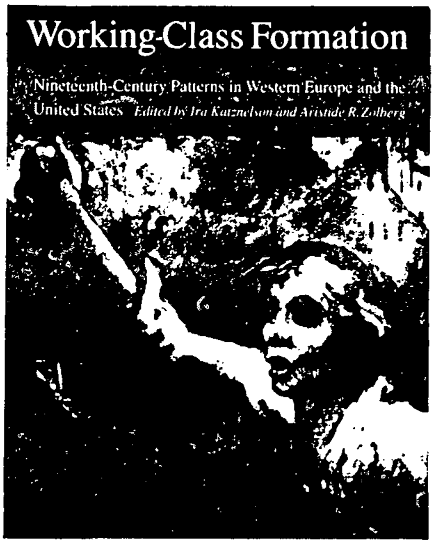
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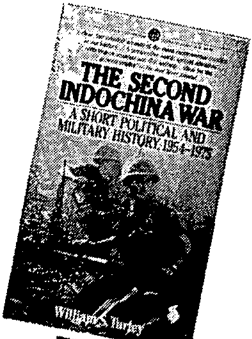
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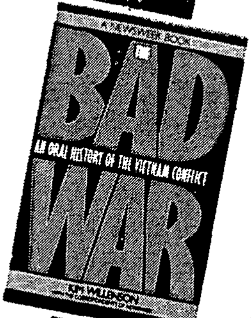
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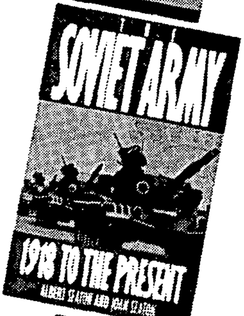
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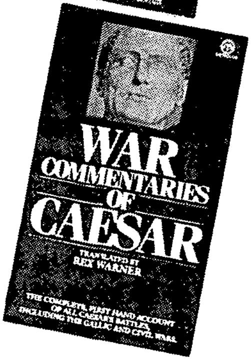
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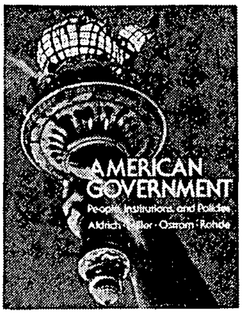
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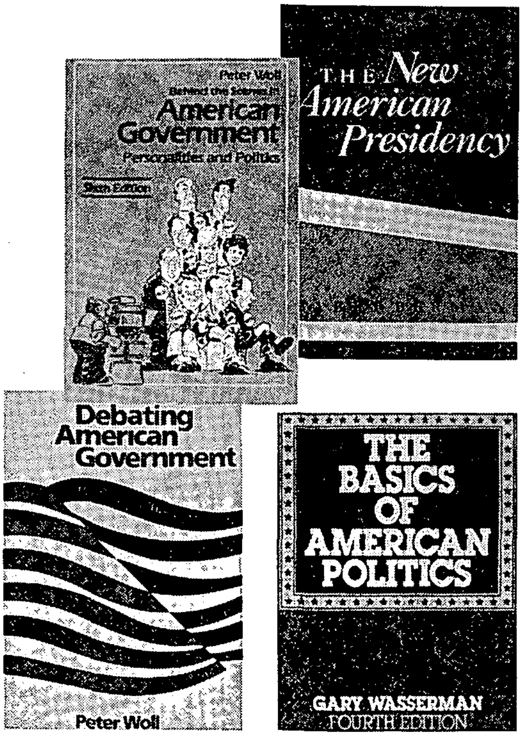
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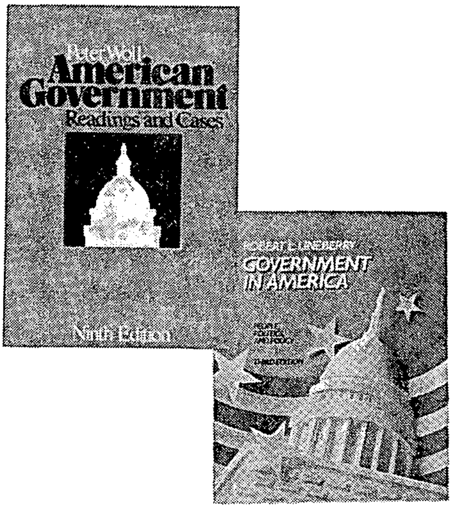

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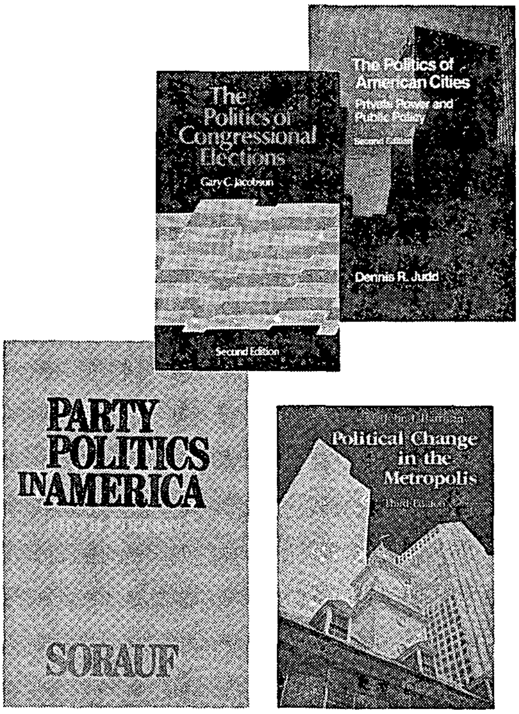
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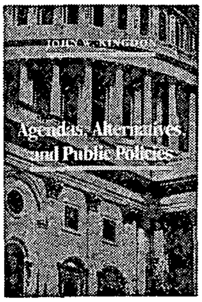

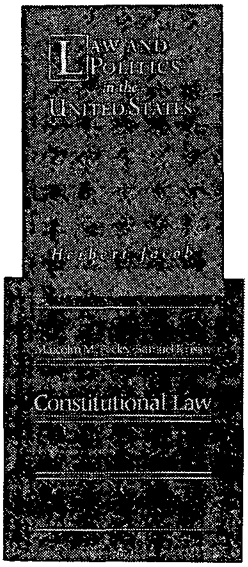
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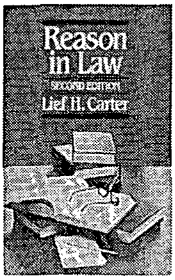

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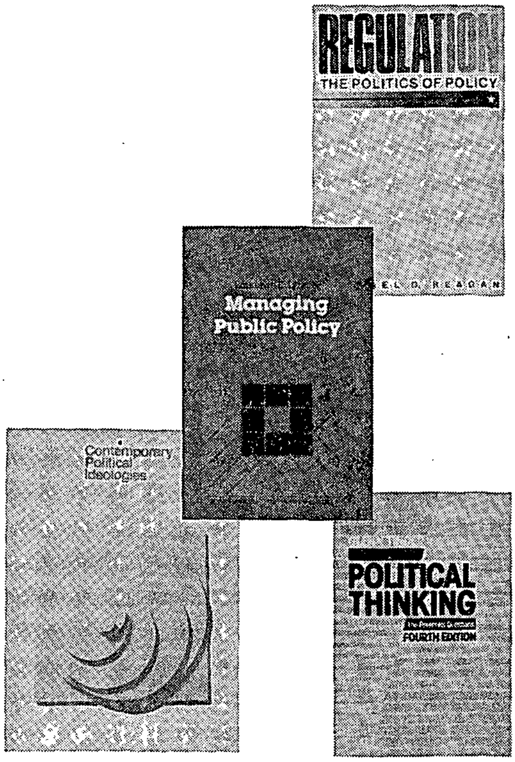

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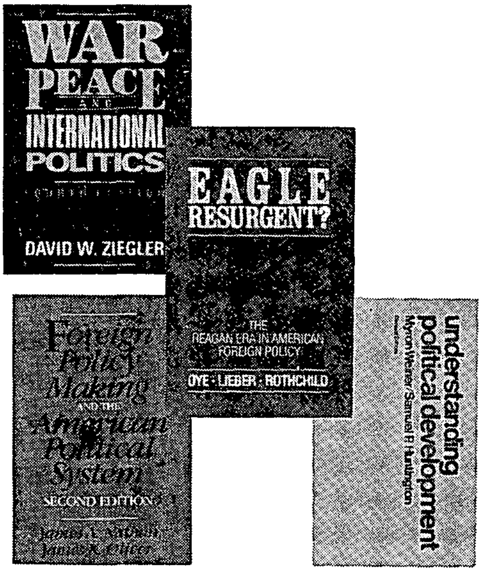

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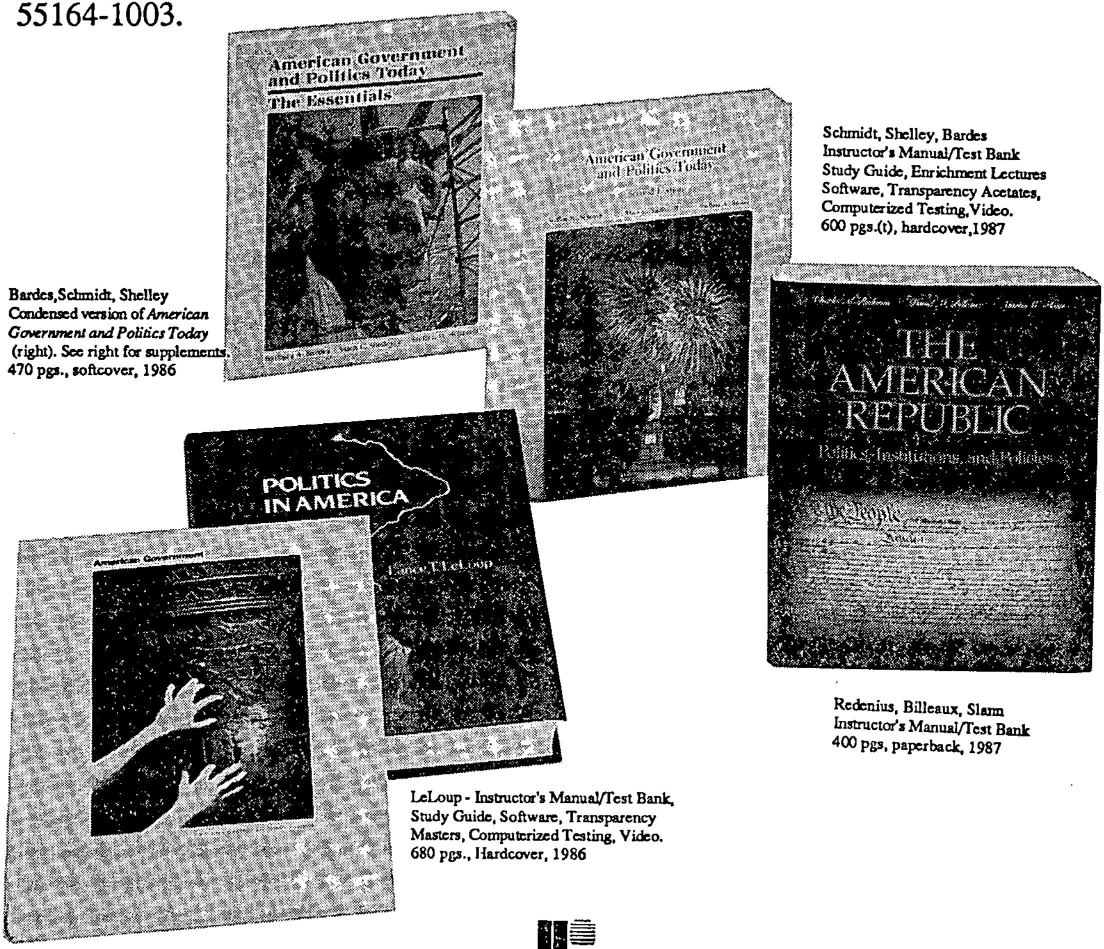
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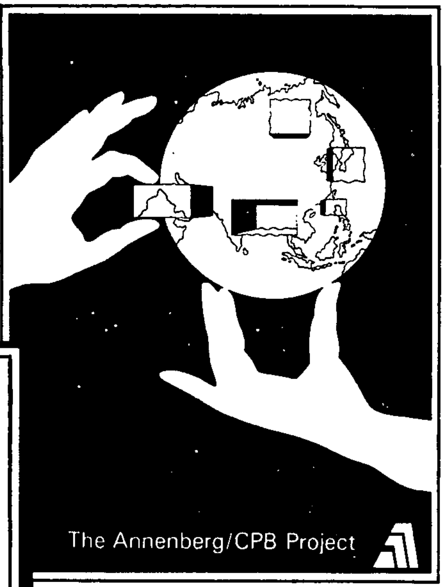

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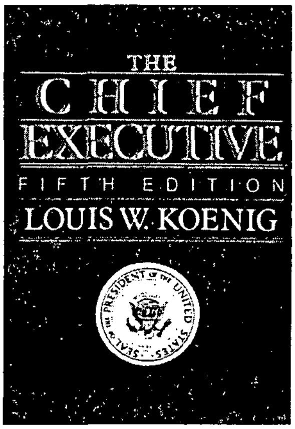
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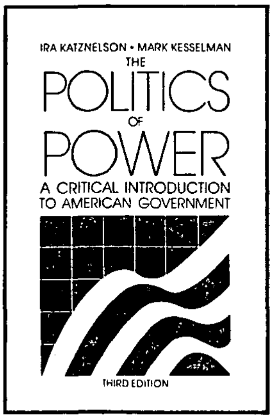
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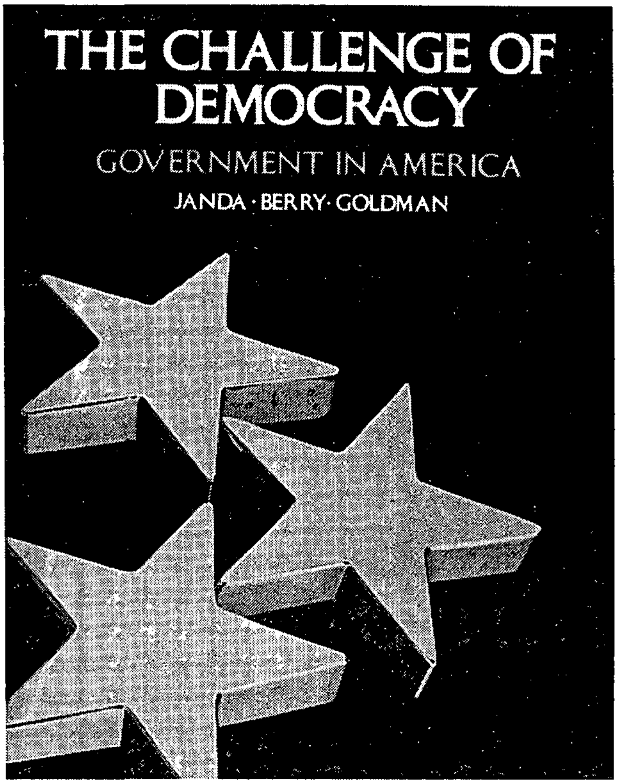

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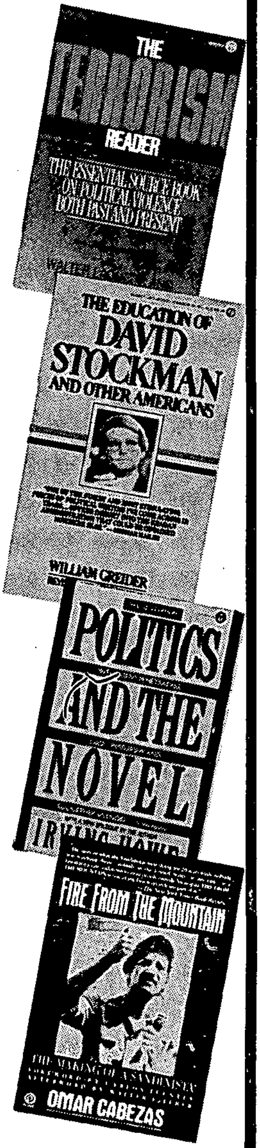
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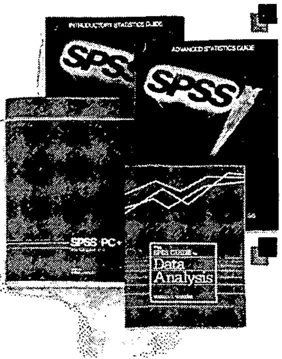

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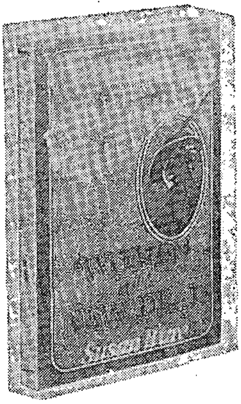

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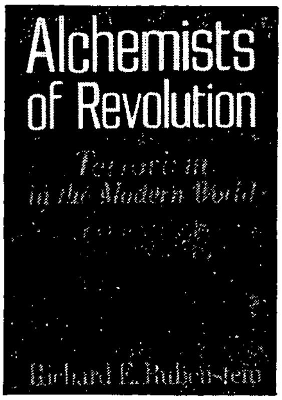

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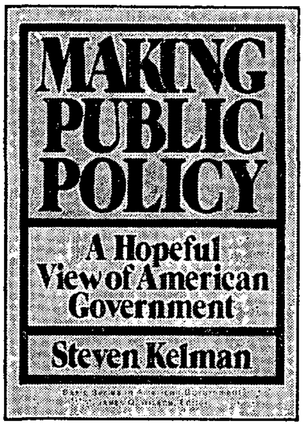

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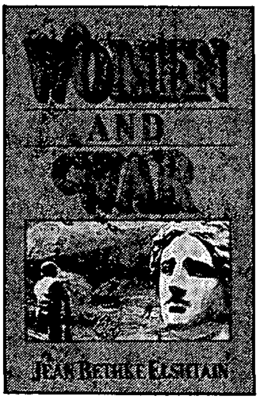

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